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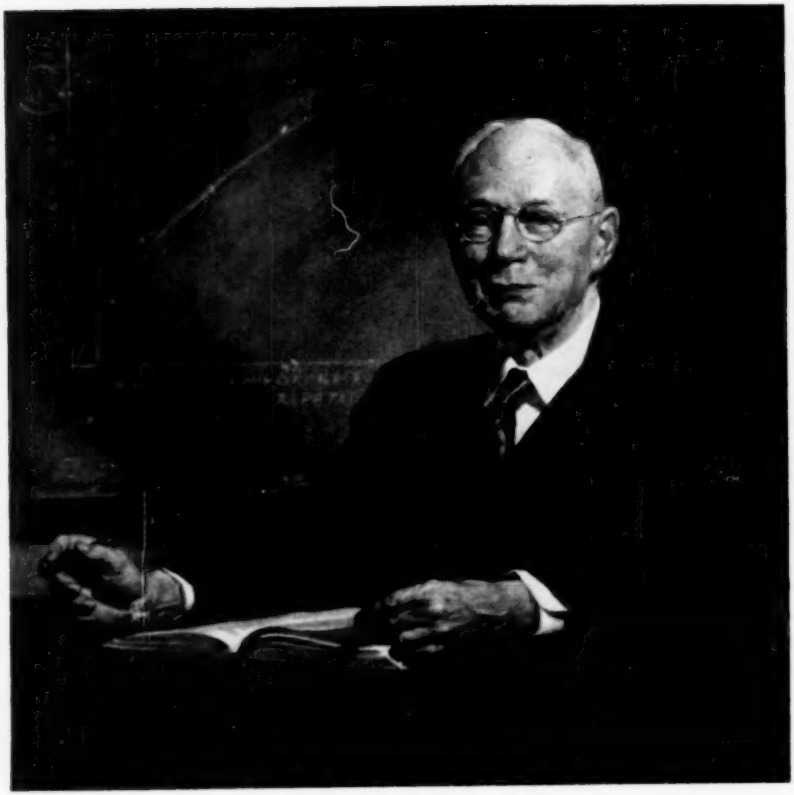

Clark L. Hull

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THE PSYCHOLOGICAL REVIEW

CLARK LEONARD HULL

1884–1952

In the death of Clark Hull, on May 10, 1952, psychology has lost one of its most distinguished contributors to research and theory. He was just completing his final year of academic service, as Sterling Professor of Psychology at Yale. A few weeks earlier he had finished and sent to the publisher the manuscript for his new book, *A Behavior System*, and had only recently been honored on the occasion of the presentation to the University of his portrait, the gift of his colleagues and former students. A photograph of this painting, which was done by Deane Keller, is reproduced on the opposite page. It shows him, as many of his students will like to remember him, conducting one of his famous seminars on behavior theory.

Clark L. Hull was born on a farm near Akron, New York, on May 24, 1884. Most of his boyhood was spent in Michigan, where he attended a rural grammar school and then Alma Academy and Alma College. His education was interrupted first by a period during which he worked as an apprentice mining engineer at Hibbing, Minnesota, and then by a protracted convalescence from an attack of poliomyelitis. After recovering from this illness, he and his bride, Bertha Iutzi, took up their studies at the University of Michigan. Here an important influence on his development was the course in experimental psychology taught jointly by Professors W. B. Pillsbury and J. F. Shepard. He received his bachelor's degree in 1913, and during the following academic year taught in a state normal school in Kentucky. It was at about this time that he decided to make psychology his life work.

His graduate work was done at the University of Wisconsin, primarily under Professors Daniel Starch and V. A. C. Henmon. He served as an assistant to Professor Joseph Jastrow. While carrying on a heavy program of assisting and teaching, he spent all of his spare time in the laboratory, working on a series of problems concerning concept formation. These studies, which became his dissertation on "Quantitative Aspects of the Evolution of Concepts," aroused unusual interest and had a considerable influence not only on his own subsequent work but on the entire development in America of studies of the higher mental processes. He continued on the teaching staff at Wisconsin after receiving his Ph.D. in 1918.

Hull's scientific work comprised three different phases, each of which constituted what other men would be proud to consider the work of an entire lifetime. His first career was concerned with aptitude testing. His interest in this field began when, on taking over Professor Starch's course on psychological tests, he found that most of the prevailing works on tests were weak in methodology and too little concerned with validation. With characteristic energy he set about the task of organizing

the field systematically, finally incorporating his efforts in a book entitled *Aptitude Testing* (1928). This work is still widely quoted in current textbooks in the field, primarily on the basis of its contributions to the analysis of validation criteria and its development of methods of transmuting ranks and ratings into scale scores. In connection with his work in this area, Hull developed the Wisconsin Lathe Test, which has become the classic example of an "analogous" aptitude test. He felt handicapped in working on test development by the protracted and tedious procedures necessary to compute tables of test intercorrelations. To reduce the time for these operations and increase their accuracy, he developed a "correlation machine," which computes squares and cross-products automatically on the basis of information supplied by paper tapes onto which the basic data are punched. This piece of equipment is still in operation.

The second research career began when he took over from Professor Jastrow a course in psychology for premedical students. In teaching this course Hull became interested in the phenomena of hypnosis and suggestibility, which he believed to be active on a large scale in medical practice. He introduced rigorous quantitative methodology into an area which had heretofore aroused great interest but little systematic study. Within a ten-year period he and his students had completed an impressive program of over thirty researches which laid the groundwork for a truly scientific account of the phenomena. A summary of these studies was prepared under the title, *Hypnosis and Suggestibility: An Experimental Approach*. This book, published in 1933, is still widely studied.

Professor Hull's third career, and the climax to his life's work, was in the field of learning and behavior theory. This might be said to have begun in 1929, with the publication in this JOURNAL of his article, "A Functional Interpretation of the Conditioned Reflex." This was the year he was brought to Yale by President James R. Angell to strengthen the newly created Institute of Psychology, which shortly became part of the Institute of Human Relations. Here he began a systematic program of research seeking to develop general laws of behavior and to determine quantitative relationships among the variables involved. The research of Pavlov and the theoretical writings of Watson had left a deep impression on him. In his Autobiography he states that around 1930 he came to the "definite conclusion" that "psychology is a true natural science; that its primary laws are expressible quantitatively by means of a moderate number of ordinary equations; that all the complex behavior of single individuals will ultimately be derivable as secondary laws from (1) these primary laws together with (2) the conditions under which behavior occurs; and that all the behavior of groups as a whole, i.e., strictly social behavior as such, may similarly be derived as quantitative laws from the same primary equations, possibly with the addition of a small number of other laws concerned with groups as such." During the next twenty years Professor Hull worked with great zeal and effectiveness in developing such laws and equations. From his own laboratory there came forth a steady stream of experimental investigations in learning, often carried out by students inspired by suggestions made at his weekly seminars. Furthermore, he had an unusual knack of getting his students so involved with their research problems that they continued related investigations when they took jobs at other institutions, and soon had students of their own carrying on similar

research. There are thus a large number of enthusiastic fourth and fifth generation students throughout the country.

Stemming from his experimental work, there developed the theoretical concepts which today form the basis for one large segment of learning theory. These include the concepts of "goal gradient," "habit-family hierarchy," "reactive inhibition," "effective reaction potential," and a host of others. A first progress report on his theoretical developments was the subject of his presidential address to the American Psychological Association in 1936, at Dartmouth. A set of mimeographed sheets presented in connection with this talk contained his first miniature system, with definitions, postulates, and theorems derived in the manner of proofs in geometry. A well-documented and more complete extension of this theory was presented in his epoch-making *Principles of Behavior* (1943), one of the most widely-cited books in psychology. The final account of his work is given in his latest book, *A Behavior System*, which will appear this fall. Of his more than fifty articles in this field, it is almost certain that the ones he considered to be of greatest significance were those concerned with the quantification of habit strength, in which he used methods derived from the scaling procedures of Thurstone.

In addition to developing his own program of research, Professor Hull had a profound impact on the work of the entire Institute of Human Relations. Not only was his example of systematic research inspiring, but his theoretical system had obvious implications for a wide variety of problems in psychology and related social sciences. He exerted great influence also through his weekly all-university evening seminars, which were attended by a large number of physiologists, philosophers, psychiatrists, anthropologists, and sociologists, as well as by psychologists. Particularly important was the series devoted to the interrelationships between psychoanalysis and behavior theory. These seminars, conducted in 1936 and 1937, were influential in directing attention to this important area, and have led to significant work at the Institute and elsewhere on problems of neurosis, conflict and psychotherapy.

Almost all of Hull's time and energy was spent on his research and teaching. He showed great reluctance in accepting membership on committees and in participating in the multitude of other professional chores which often take so much of the time of other psychologists. When he did accept, however, his work was done in an extremely conscientious and thorough manner. His major activities in the APA were in connection with membership on the Council, in 1931–33, and with serving as the forty-fourth president of the association during 1935–36. He attended two international congresses, one at Copenhagen, in 1932, and one at Paris, in 1937, and he thoroughly enjoyed these adventures. A number of scientific societies have honored him. He was elected to the American Academy of Arts and Sciences in 1935, and to the National Academy of Sciences in 1936. One society whose meetings he greatly enjoyed was the Society of Experimental Psychologists, which he attended regularly. He was awarded the prized Warren Medal by this society in 1945, with the citation:

"To Clark L. Hull: For his careful development of a systematic theory of behavior. This theory has stimulated much research and it has been developed in a precise and quantitative form so as to permit predictions which can be tested empirically. The theory thus contains within itself the seeds of its own ultimate verification and of its possible final disproof. A truly unique achievement in the history of psychology to date."

To those who did not have the good fortune to know him well, Professor Hull often seemed somewhat reserved and austere. But all who came into more frequent contact with him found him readily accessible, genuinely concerned with their problems, and always greatly interested in the subsequent careers of those whom he had come to know. One of his most marked characteristics was his never-failing appreciation and acknowledgment in his writings of any assistance in his work which he received from others. His personal tastes were simple, and his greatest vice was a proclivity to bet malted milks on the outcome of experiments. To all who knew him his tireless and selfless devotion to science and his high personal sincerity, loyalty, and integrity were a genuine source of inspiration.

The full magnitude of Hull's contribution to experimental and theoretical psychology cannot at present be evaluated. But it is reflected quantitatively in the unusual frequency with which his work is cited. In the recent *Handbook of Experimental Psychology*, for example, he leads all other scientists in the number of references made to him (his work is mentioned on over eighty pages). Reading of the last two years' issues of the *Journal of Experimental Psychology* and of the PSYCHOLOGICAL REVIEW reveals that over forty per cent of the bibliographies in these two journals included one or more of his writings (and in the articles dealing with Learning the figure is closer to one hundred per cent!). His contribution qualitatively is fittingly expressed in the memorial written by Harold Schlosberg on behalf of the Division of Experimental Psychology of the APA:

"Experimental Psychology has suffered a great loss in the death of Clark L. Hull. It is difficult to imagine what our field would have been today if we had not had his brilliant theoretical analyses and his stimulating personal leadership during the last quarter of a century. But it is one of the marks of his genius that he will live on in the work of his students, his associates, and indeed, of all those who labor in the broad field he cultivated so effectively. If they carry on his selfless devotion to the progress of scientific psychology, they will be an appropriate living memorial."

CARL I. HOVLAND

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FRUSTRATION AND THE QUALITY OF PERFORMANCE: I. A CRITIQUE OF THE BARKER, DEMBO, AND LEWIN EXPERIMENT

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INTRODUCTION

The Freudian concept of regression has given rise to varied research and theoretical work. In particular, it has on at least two occasions given rise to an attempt to bring the concept and the phenomena of regression within the scope of another theoretical system than that of psychoanalysis. In each case the attempt to do this is characterized by a redefinition of the concept of regression and by an account of the dynamics responsible for regression, as interpreted in relation to the particular theoretical system.

One of these attempts is that of Mowrer (5) and later Sears (8), relating regression to behavioristic theory of learning. They define regression, in effect, as the reappearance of behavior which earlier characterized the person, when more recently acquired behavior disappears under frustration. The account of the underlying dynamics is as follows. A particular situation will have some tendency to evoke a number of different response-sequences in an individual. The response-sequence for which this tendency is strongest will always be the one which is actually evoked; this will quite commonly be the most recently learned sequence. If frustration occurs, the strength of the tendency for evocation of this particular sequence will be diminished through the process known as extinction. When it is diminished below the strength of the tendency for evocation of the response-sequence of second greatest strength, this latter sequence will be evoked. This sequence will often be an earlier learned one. Thus does frustration lead to regression. Mowrer appears to feel that the variety of phenomena which have been termed "regression" by Freud fit adequately this definition and explanation.

The other attempt, relating regression to the Lewinian system, is that of Barker, Dembo and Lewin (2).[1] They define regression, in effect, as the primitivation of behavior, as a shift from more mature behavior to less mature behavior. The account of the underlying dynamics is roughly as follows. Under frustration (or at least strong frustration) the person becomes less highly or less adequately organized, and his behavior is therefore more like that of a less mature person than it was before frustration. Actually several different dynamic sequences are suggested by BDL as possibly responsible in various cases, but the previous sentence seems to be a fair summary of them. These several sequences have in common that what becomes less mature is the *general* degree of organization of behavior, rather than *specific* acts or response-sequences, and that resemblance of any kind to *earlier* behavior is not an essential part of the concept.

These two redefinitions of "regression" clearly involve very different concepts—one, the reappearance of specific response-sequences which were learned earlier than the frustrated sequence; the other, a change in the organizational properties of behavior and its internal determinants. In our opinion, neither of the concepts corresponds very closely

[1] Hereafter referred to as BDL.

to the total meaning of "regression" as used in psychoanalytic writing, though each seems to correspond to part of the psychoanalytic meaning. Our opinion on this point appears to have been shared by Sears (8, pp. 96–98) and by BDL (2, pp. 2–7).

In this series of papers, we are concerned with essentially the same concept for which BDL have employed the term "regression." But because of the great variation in the meaning of this term, and because the psychoanalytic usage seems to be the most widely accepted, we feel that to continue to use the same term would be confusing.

In our choice of a term to substitute for "regression" we are guided by the fact that we are concerned not only with this phenomenon but also with its opposite. We are interested not only in what leads behavior to be less organized but also in what leads behavior to be more organized. A term would be useful which would call attention to the possibility of variation in either direction. The term we have adopted is "quality of performance." We shall be concerned, then, with the opposite effects that frustration may have on quality of performance—the raising and the lowering of quality of performance.

This term is, we admit, somewhat general and vague. In the case of clearly defined intellectual tasks such as are used in intelligence tests, quality of performance may readily be given meaning through highly reliable judgments of correctness of solution, speed, and the like. In the case of systematic observations of a category of behavior, such as BDL's observations of children's play in a standardized situation, criteria can be worked out for obtaining reliable judgments of an aspect of the behavior that could be called "quality of performance"; BDL have done just this, though they have used the narrower term "constructiveness." In applying our concept to everyday behavior, there would often be difficulty in evaluating the quality of performance. None the less, we believe that even here the concept would have a sufficiently common meaning to justify our devoting attention primarily to the factors influencing quality of performance, rather than to the development of a more refined set of concepts which might ultimately replace this general one.

Of experiments concerned with the effects of frustration on quality of performance, that by BDL is perhaps the most ambitious and the best known. The great wealth of data and ideas contained in their lengthy report of the experiment, unfortunately, do not seem to be nearly as well known as a general conclusion drawn from their report—the general conclusion that frustration leads to a lowered quality of performance. When the data and ideas presented by BDL are looked at in detail, other inferences may be drawn that are much more significant than this general conclusion—more significant for an understanding of their study, and more significant for the orienting of future research. Hence we feel that a critique of this major piece of scientific work is fully justified.

THE BARKER, DEMBO, AND LEWIN EXPERIMENT

A critique of the BDL experiment must begin, for those who are not familiar with it, with a brief account of the experiment itself. The subjects were children between the ages of 25 months and 61 months. The experiment was conducted in a room which could be divided by a screen into two parts, which we shall call parts A and B. For the first session with each child the two parts of the room were separated by a completely opaque screen. The child was brought into part A and

allowed to play with a set of play materials which we shall call set A; this session is referred to as the free-play situation. On a later day the child was brought back into the room after two changes had been made in preparation; the barrier had been removed, so that the child had free access to the entire room; and in part B there were a number of much more attractive toys, which we shall call set B, with which set A had also been integrated. When the child had become fully involved in play in this situation, the frustration situation was introduced in the following way: The toys of set A were withdrawn from those of set B and placed in their original position in part A of the room; the child and experimenter withdrew into part A, and the experimenter lowered a transparent screen (of wire netting) between the two parts of the room and padlocked it. The child was then objectively in the same physical situation as in the free-play situation of the first day, except for the visibility of the more desirable toys, those of set B. The child's play with the toys of set A was rated on a scale of constructiveness,[2] both in this frustration situation and in the free-play situation of the first day.

Most of the children showed a lower constructiveness of play in the frustration situation than in the free-play situation.[3] This finding appears to us to be capable of a very simple and very illuminating interpretation. It is that frustration of one activity will produce lowered quality of performance in the second activity to the extent that it leads to the making of responses that are incompatible with or interfere with the responses of the second activity. This may be called an interference interpretation of the findings. It will be considered at somewhat greater length in the next section of this article. Meanwhile, we wish to consider its relation to BDL's interpretation of their findings.[4]

This interpretation we are offering is not entirely new. In a short paper published by Barker alone (1) in advance of the full report of the experiment, essentially this interpretation is presented, and it is the sole interpretation offered there for the lowered constructiveness found in the experiment.[5] Essentially

[2] To ensure faithfulness to the statements by BDL, we shall continue to use the term "constructiveness" in discussing their experiment. In our opinion, their scale of constructiveness is an instance of a scale of what we are calling "quality of performance."

[3] Some children showed instead a heightened constructiveness. The problems to which these exceptions give rise will be discussed in a later paper (4).

[4] BDL's experiment is, unhappily, subject to still another and quite different interpretation. This is that the lowered constructiveness of play with the toys of set A was due entirely to satiation with respect to them. The experiment was so designed that there is no control for the influence of satiation. BDL argue that this interpretation may be rejected because the more frustrated individuals showed a greater lowering of constructiveness than did the less frustrated individuals and hence frustration must have been responsible. This argument is not conclusive, since the measure which is called a measure of degree of frustration could with equal justice have been called a measure of degree of satiation. We are influenced, however, by the qualitative reports of behavior, as BDL must have been by their observation of the behavior, to believe that frustration probably had *something* to do with the lowered constructiveness; how much, in relation to the influence of satiation, there is no way of guessing from the data of the experiment.

[5] In saying, "essentially this interpretation," we are, of course, making a sort of translation between Lewinian constructs and behavioristic constructs. We believe the translation is a reasonable one in this case, and that the arguments which apply to our interpretation apply equally to the analogous interpretation as stated by these Lewinian authors. Hence, we have felt justified in referring, in the rest of this paper, to an interference interpretation even when we are discussing these other authors' statements about an interpretation which they describe in very different terms.

the same interpretation is offered in the full report by BDL, in a section entitled "A Simplified Quantitative Theory of Constructiveness" (pp. 212–216)[6] and elsewhere. But at the same time these authors argue against this interpretation. In a briefer report of the experiment later prepared for inclusion in a textbook (3), the same authors relegate this interpretation to an even more subordinate and inconspicuous position.

Let us consider, then, the justice of the denial or minimization of this interpretation in the reports of BDL. There are two separate arguments to consider. The first is the minimization of this interpretation by giving it equal status with a number of other alternative interpretations. The second consists of specific arguments against the supposition that an interpretation in terms of interference might adequately explain the whole of the findings.

BDL minimize the role of interference in explaining lowered constructiveness by presenting it as one of four possible sequences of internal events which are given equal explanatory status. We shall comment below on the other three of these sequences, reserving for the next section of this paper extended discussion of the interference interpretation.

(1) It is suggested (pp. 44–45, 217) that frustration produces a dedifferentiation of the person, probably because frustration causes tension and tension causes dedifferentiation, and that this dedifferentiation of the person leads to lowered constructiveness of play. This hypothesis cannot be altogether rejected. But its application to explaining the results of the experiment involves circular reasoning, since no criterion of dedifferentiation is used other than the lowered constructiveness of the play. Moreover, if variety of total behavior were used as the criterion for degree of differentiation, it seems quite certain that frustration would be found to have led to increased differentiation instead of the opposite, for the total behavior in the frustration period appears to have been much more varied than in the free-play situation, even though the play itself was less varied.

(2) It is suggested (p. 217) that frustration leads to keeping a part of the whole person in a fixed state, and that this leads to a decrease in the variety of behavior and hence to lowered constructiveness. The same objections apply here as in the former case. With respect to a part of the person being kept in a fixed state, there is no operation suggested for determining whether this has happened and no evidence that it would be true if a suitable operation were used. With respect to the consequent lowered variety of behavior, we would raise the same doubts as in the previous paragraph as to whether the facts were not directly contrary to what is suggested.[7]

(3) It is suggested (p. 218) that in the free-play situation the child was free to make long-range plans for play because he had no reason to expect to be interrupted, whereas in the frustration situation he had just experienced an interruption of his play by the experimenter, might have expected another interruption and hence "it might not seem worth while to start a long-

[6] This and subsequent page references are to the BDL monograph (2).

[7] We find the meaning of this second interpretation especially difficult to grasp, and would suggest the possibility that upon further clarification "keeping a part of the whole in a fixed state" might turn out to mean only that there was a continuing system of responses which would tend to interfere with any subsequent responses. In this case, this second interpretation is actually identical with the one we are supporting, but the mention of variety of behavior should be omitted from it.

range plan." This lack of security is then supposed to lead to a lowered constructiveness of play. This hypothesis is clearly meaningful and tenable. It may account for some or all of the lowered constructiveness of play. The experiment was not designed in a way that would permit certainty on this point. Yet, as in the case of the interpretation in terms of satiation which has been discussed above in footnote 4, we are swayed by certain relevant evidence. Evidence that the children's play was being interfered with by other behavioral tendencies is simply overwhelming, as will be shown later. Evidence that the children expected to be interrupted is lacking; that the authors suggest this interpretation but do not cite any evidence to support it tends to suggest that there was not evidence of its being very important.

Against all three of these alternative interpretations there is an argument to be made from the evidence of the experiment which is rather persuasive though not conclusive. All three of these interpretations refer to rather general, pervasive states of the person during the frustration period which might be expected to influence the constructiveness of various categories of his play activity. Each separate incident of play was in fact classified by the investigators into one of two categories, primary play and secondary play. Primary play is play in which the child was, so far as could be judged from watching him, entirely absorbed. Secondary play is play in which the child was simultaneously doing something else, such as talking to the experimenter or looking at the more desirable toys. The constructiveness of primary play is markedly different in free play and in frustration. The constructiveness of secondary play shows no consistent difference between free play and frustration.[8] When the child's play is suffering from interference in a way that is clearly visible to an observer, then none of the factors mentioned in these three alternative interpretations, supposedly present in the frustration period more than in the free-play period, is sufficient to produce any additional decrement in constructiveness.

There is, on the other hand, considerable evidence in favor of the interference interpretation which we shall review later in this paper. We conclude, therefore, that BDL misplace emphasis in giving equal weight to it and to these three other interpretations. The experiment itself is not dècisive with respect to the several interpretations, but it can provide qualitative evidence about their relative plausibility and compellingness as major explanations.

A reason for the lack of emphasis by BDL on the interference interpretation is probably to be found in the fact that they believe they have definite evidence against its tenability as a sole explanation of their findings. To this second point we now turn.

There are three lines of evidence which BDL quite explicitly apply against the interference interpretation, and a fourth which they seem to apply also. Let us consider each of them.

(1) It is argued (pp. 131–134) that because the lowered constructiveness in frustration cannot be accounted for entirely by an increase in the proportion of secondary play (i.e., play which is accompanied by the child's simultaneously making other overt responses) and is still found when a comparison is made between primary play in the free-play and the frustration situations, the interference interpretation cannot be the sole explanation. This argument appears to involve the assumption that primary play (i.e., play in which the

[8] This fact is not mentioned by BDL but may be read from their Table 6 (p. 119).

child was not obviously and overtly doing something else at the same time) had the same meaning in both free-play and frustration; that is, that in both situations, it involved no internal conflict, no preoccupation with competing interests. This assumption seems to us altogether unlikely from a commonsense analysis of what had been done to the children in the two situations and of what their probable reactions would be, and from other statements by BDL (pp. 78–81). It involves a naive objectivism of a degree which would be rather surprising in a behaviorist, and in these authors is quite astonishing.

(2) It is argued (pp. 147–152) that because the most strongly frustrated children showed a greater lowering of constructiveness than did the less strongly frustrated children, frustration rather than conflict (or satiation) must be the explanation. But here it is necessary to note the particular measure which was used in gauging the strength of frustration in each child. It was the proportion of the total experimental period which he devoted to barrier and escape behavior. But this is at the same time a measure of the extent to which certain behavior tendencies were strong enough to win complete dominance, for periods of time, over the tendency to play with the available toys of set A.[9] Surely a measure of strength of competing tendencies based on the proportion of time in which they are dominant may be expected to provide an index also of their strength during the periods in which they are not dominant! Hence what the investigators have chosen to consider a measure of degree of frustration could with equal cogency be called a measure of strength of competing tendencies. We can for this reason see no merit at all in this particular argument by BDL.

(3) Another argument is based on the fact that the maximum of constructiveness reached at any time during the frustration period is typically lower than the maximum of constructiveness reached at any time during the free-play period. This argument is expressed very concisely by Barker, Dembo and Lewin.

". . . these results . . . suggest that even with play of highest constructiveness, where the probability is smallest that overlapping regions of play and nonplay are involved, there is a tendency for constructiveness of play to be reduced under the influence of frustration. This is further evidence, therefore, that division of the person between two simultaneous actions is not the only cause of the reduction of constructiveness in the frustration situation" (pp. 135–136).

We cannot see the cogency of this argument either. If the interference interpretation is correct, then the facts referred to here do indicate that the competing tendencies to get through the barrier, to escape, etc., were characteristically present throughout the frustration period. But there is nothing farfetched about the supposition that these competing tendencies were present more or less continuously. In fact, the whole experimental procedure was planned in an effort to make this supposition correct, and BDL seem elsewhere in their report to assume that it is correct (pp. 58–59, 71–82).

(4) It seems also to be argued that the fact that lowered constructiveness is found even when units of play of equal length only are compared contributes some evidence against the interference interpretation (p. 152). Again, we cannot agree with this argument, if we are correct in believing it to be implied.

[9] This measure is, of course, only of the *relative* strength of the competing tendencies; it is possible that the variations arise from differences in the absolute strength of the tendency to play with the available toys rather than from differences in the absolute strength of the competing tendencies. (See footnote 4 above.)

The fact cited here is important as showing that the effect of interference on constructiveness is not produced solely by shortening the duration of each unit of play. But there is nothing about the interference interpretation, either as we understand it or as it is presented by BDL, that would require that interference be supposed to produce an effect in only this one way.

We would conclude, therefore, that the arguments adduced by BDL against the interference interpretation of their results are not valid.

EVIDENCE FOR THE INTERFERENCE INTERPRETATION

We offer the hypothesis that frustration of one activity effects a lowering of quality of performance in a second activity to the extent that it gives rise to competing responses which interfere with the responses involved in the second activity. The interfering responses may usefully be divided into two sorts:

(1) Responses made in continuation, or attempted continuation, of the frustrated activity. These are what Rosenzweig (7) terms need-persistive responses to the frustration. Frustration implies that the individual has not reached, or has not completed his enjoyment of, his goal; and frustration hence may be followed by continued attempts to reach the goal or by substitutive goal-responses such as fantasies about enjoying the goal or striving for the goal. All these responses, made with reference to the goal of the frustrated activity, may interfere with the responses required for high-quality performance in a second activity.

(2) Responses evoked more specifically by the fact of frustration itself. These include what Rosenzweig calls *ego-defensive* responses to frustration, and also some of the internal drive-producing responses which occasion the defense. They include, for example, the internal responses which constitute anxiety and anger, aggressive responses made to these conditions, attempts at self-justification, attempts to escape from the unpleasant situation, and the like. Such responses, evoked by frustration in one activity, may interfere with high-quality performance in a second activity.

We suggest that the lowered constructiveness of play found in the BDL experiment is adequately explained by the interference of responses of these two sorts with the responses of playing with the available toys of set A during the frustration period. We feel also that some of the best experimental evidence at present available for this general hypothesis is provided by the BDL experiment. It is appropriate, therefore, to review that experiment from the point of view of the positive evidence supporting this interpretation of its findings. This evidence is presented below under four main headings.

I. *An Analysis of the Situation*

An analysis of the situation with which children were confronted in BDL's experiment makes it extremely likely that the children's play in the frustration period would be subjected to a great deal of interference; that is, that there would be evoked strong tendencies toward behavior which would compete with the responses of playing with the available toys of set A. The salient points in such an analysis appear to be the following:

A. The children were interrupted at a point of thorough involvement in play with the combined toys of sets A and B. From the results of experiments on resumption of interrupted tasks (6), it would be expected that this would lead to definite persisting tendencies to resume the interrupted activity. Since, moreover, the interrupted activity in this case appears to have been much

more desirable than the ones which have been generally used in those experiments, one might expect the persisting tendencies to be very strong indeed.

B. The more desirable toys of set B remained clearly in sight, likely to evoke tendencies to respond to them and attempts to secure the opportunity of continuing to play with them.

C. There was a special circumstance in the social situation which seems likely to have strengthened the tendency to get across the barrier, to play with the toys of set B. There was no definite statement to the child that the frustration would be permanent or would last for any given time; the situation might for this reason appear to the child as similar to everyday situations in which some behavior on his part could lead to the removal of a socially imposed barrier.

D. If he played with the available toys of set A, he was playing with toys which had just previously been integrated with the more desirable toys of set B, and might therefore remind him repeatedly of his unfinished play with set B.

E. There was no special urging that the child play with the toys of set A. No motives of conformity or achievement were thus evoked by social stimulation. The tendency to play with these toys must have arisen from the general desire for play or for occupying oneself. Since these toys were so much less satisfying than those the child had just been playing with, the tendency to play with them, unsupported by adult urging, might be expected to be relatively weak and hence to require not very strong competing tendencies for effective interference.

F. The situation was one in which the alternative of escape was likely to be a relatively attractive one, as the child evidently knew that when he could get away from this situation he would be returned to his accustomed and presumably enjoyed activities in nursery school. Hence a tendency to escape might be expected as a relatively important component in the conflicts created by the experimental situation.

II. *Evidence of Increased Interference in the Frustration Situation*

The investigators' observations of the children's behavior, and their analysis of it, provide evidence that play with the toys of set A was in fact interfered with by other overt behavior, more during the frustration period than during the free-play period. It seems a reasonable inference that what was true of overt interference was also true of covert interference. Two lines of evidence are pertinent here.

A. *Time devoted to play and to other activities.* During all of the free-play and frustration periods, the toys of set A were available for the child to play with. To what extent in each period was play interfered with by other overt activity? A simple answer is provided by the amount of time devoted to play and to other activities. This information may be calculated from data in two of BDL's tables (pp. 85–86). The average amount of time devoted to primary play in the frustration period was just half what it was in the free-play period—570 seconds against 1144 seconds. The amount of time devoted to all the other recorded activities was more than three times as great in the frustration period as in the free-play period—827 seconds against 246 seconds.

B. *Proportion of play which was "secondary."* There is a second way of answering the question, to what extent was play interfered with, in each period, by other overt activity? This second answer is provided by BDL's observations on the amount of what they call "secondary play." In the free-play period the child was simul-

taneously playing with the toys and doing something else 2.3 per cent of the time, on the average; for the frustration period the corresponding figure is 8.9 per cent (p. 87). In view of the fact that there was only half as much primary play in the frustration period, it appears that the proportion of playing time in which other behavior was visibly intruding on the play was about 7 or 8 times as great in the frustration period as in the free-play period.

It is perfectly clear, then, that other behavioral tendencies which might interfere with play were very much stronger, relative to tendencies to play, during the frustration period than during the free-play period. It is natural to suppose that this fact is pertinent to explaining the difference between the two periods in constructiveness of play, or quality of performance.

III. *Evidence of Correlation between Interference and Quality of Performance, in the Frustration Period*

We have just seen that the difference in quality of performance between the free-play period and the frustration period may well be explained in terms of interference, inasmuch as there is ample evidence that interfering activities were present to a much greater degree during the frustration period than during the free-play period. We may now turn to another line of evidence which may be drawn from the BDL monograph to support the same interpretation. It is evidence deriving from the data of the frustration period alone, evidence indicating that within that period alone variations in quality of performance are correlated with variations in the extent to which interfering responses are being made. This evidence comes from three specific sources.

A. *Comparison between primary and secondary play.* For children's play during the frustration period, the ratings of constructiveness were higher for "primary play" than for "secondary play"; this finding is highly significant statistically ($t = 4.72$, d.f. $= 19$, as computed from data in Table 6, p. 119). Since the difference between primary and secondary play is a matter of whether response tendencies which might compete with play are present in sufficient strength to have some directly observable effect, this finding provides direct evidence of a correlation between interference and quality of performance.

B. *Individual differences.* BDL divide their subjects, for purposes of analysis, into two groups—10 weakly frustrated subjects and 20 strongly frustrated subjects. They provide some evidence of lower constructiveness of play, and of more lowering of constructiveness in comparison with the free-play period, in the strongly frustrated subjects than in the weakly frustrated subjects. They call attention to this latter evidence, and place more stress on it than is justified by their analysis since the differences are not statistically significant (as may be determined by calculations based on their Tables 11 and 12, pp. 148–149). But the differences are certainly in this direction.

The important point here is that, as we have mentioned earlier, the "strongly frustrated" subjects could just as well have been called the subjects "with strong interfering tendencies," and the "weakly frustrated" subjects could just as well have been called the subjects "with weak interfering tendencies." The measure of frustration was the time spent by a subject in barrier and escape behavior (actually, the measure used was the increase in time so spent between free-play and frustration periods, but the division into two groups of subjects is exactly the same as though time so spent in the frustration period were the measure—see Table 9, p. 142).

Barrier and escape behavior is behavior which is competing, in this situation, with the behavior of playing with the available toys. Moreover, this division of subjects into two groups also divides them into groups differing consistently, on the average, in every other form of competing behavior itemized by BDL. The "strongly frustrated" subjects exceed the "weakly frustrated" subjects in amount of time spent in play with the experimenter, in "island behavior," in activity at the window, and in looking and wandering about (p. 145). It is a reasonable inference that subjects in whom these various other responses are dominant in more of their overt behavior will also have their play interfered with more by tendencies toward making these responses. But if this is doubted, then specific evidence may be offered: the proportion of play during which the subject is visibly making other responses simultaneously is more than twice as great in the "strongly frustrated" group as in the "weakly frustrated" group (p. 145).

Now that we have presented this interpretation of what BDL's analysis of individual differences means, let us return to the evidence. The question arises, could their differences be lacking in statistical significance simply because of the considerable variability introduced in their data by the age range of the subjects, since constructiveness of play is closely related to age? This question can readily be dealt with if we turn from an analysis of group differences to an analysis of correlations. The correlation between constructiveness of play and time spent in barrier and escape behavior, during the frustration period, is -0.33. When the effects of chronological age and mental age are partialed out, this correlation rises to -0.72, a highly significant relationship in the predicted direction.[10] We do have clear and dependable evidence from individual differences, then, to indicate that the equality of performance in play is correlated with an index of amount of interference from competing behavioral tendencies.

From our general hypothesis it might be predicted that a similar relationship would be found between constructiveness and amount of time spent in other activities than barrier and escape behavior—in play with the experimenter, "island behavior," activity at the window, and looking and wandering about. We tested this and found that the raw correlation is almost zero. One might be tempted to conclude that barrier and escape behavior has some unique predictive power; in section IV, however, we shall present evidence which argues against this conclusion.

C. *Comparison of episodes.* BDL apply to specific episodes of play an analysis similar to that used for individual differences. For each episode the constructiveness of play is rated and a measure of degree of frustration is obtained. Here the measure of "frustration" is slightly different. It is the proportion of time devoted by the subject, during the episode, to all the other responses which were recorded: barrier behavior, escape behavior, activities with the experimenter, "island behavior," activities at the window, and looking and wandering about (p. 156). There is very marked and consistent evidence that the constructiveness of play, for specific episodes, varies inversely with the proportion of time devoted to all these other activities (pp. 164–170). This finding provides strong and direct evidence of a relationship between strength of interfering tendencies and quality of performance.

[10] These calculations are based on data presented by BDL on pp. 48, 85, and 123.

IV. *Evidence of Correlation between Interference and Quality of Performance in the Free-play Period*

The simple notion that frustration produces regression does not lead to predictions about the behavior of subjects during the free-play period (unless it can be established that frustration occurred there too). Our interpretation does. For behind our interpretation is a more general hypothesis, *viz.*, that quality of performance is lowered to the extent that interfering response tendencies are present; this more general hypothesis leads directly to predictions about the behavior of subjects during the free-play period as well. Fortunately, BDL present the data of their study in such detail that it is possible to test some of these predictions by new calculations made from certain of their tables.

A. *Comparison between primary and secondary play.* Our interpretation would lead to a prediction that "primary play" should be more constructive than "secondary play" in the free-play period, just as it was in the frustration period. This prediction is very strongly confirmed by the data, and the difference is highly significant ($t = 5.03$, d.f. $= 12$, data from Table 6, p. 119). Here is further confirmation of the relation between interference and quality of performance.

B. *Individual differences.* Individual differences during the free-play period may be analyzed in the same way as individual differences during the frustration period. Our hypothesis leads to a prediction that here too the constructiveness of play should be related inversely to the strength of interfering tendencies. We have used the correlation technique again, since it permits statistical control of the influences of age. For purposes of comparison with the frustration period we have again dealt separately with two measures of interfering tendencies—time spent in barrier and escape behavior, and time spent in the other four categories of activity which were recorded.

For the free-play period the correlation between constructiveness and amount of time spent in barrier and escape behavior is only $-.06$. The correlation between constructiveness and time spent in the other four categories of activity, however, is $-.25$; when the effects of chronological age and mental age are partialed out, this correlation rises to $-.83$, a highly significant value. Once again, then, for one of the two measures of strength of interfering tendencies we obtain strong confirmation of a relationship between strength of interfering tendencies and quality of performance. This time our hypothesis is confirmed in a setting in which there appears to be no convincing reason to suppose that the concept of frustration is pertinent.

Putting together these findings and our parallel analysis for the frustration period, we may ask why different measures of interfering tendencies are significantly related to quality of performance. A partial answer may be provided by noting that in the free-play period much more time is spent in the other four activities than in barrier and escape behavior, while the reverse is true for the frustration period, and that these differences in mean time are accompanied by differences in variability among subjects. For the frustration period, barrier and escape behavior has the higher standard deviation by a ratio of 1.4 to 1; for the free-play period, the other activities have the higher standard deviation by a ratio of 2.1 to 1. In each situation, the types of interfering activity which are the major locus of individual differences have the greater predictive power. It may still

remain true that in the frustration period barrier and escape behavior has a predictive advantage not entirely accounted for in this way; but it clearly does not have a unique predictive power.

CONCLUSION

We have interpreted BDL's study of "regression" as pertinent to the following hypothesis: that frustration of one activity will produce lowered quality of performance in a second activity to the extent that it leads to the making of responses which interfere with the responses of the second activity. An interpretation which appears to be essentially the same as this was suggested by BDL but rejected by them as a sole explanation of their results. We have shown that their arguments against this hypothesis are not convincing. Various aspects of their analysis specifically support this hypothesis. We have analyzed other aspects of their data and shown that they too support this hypothesis. Finally, we have shown that the more general hypothesis which lies behind this leads to new predictions about aspects of their data which are not relevant to frustration; these too are confirmed by new analyses. We conclude that BDL's experiment provides highly significant evidence in favor of the hypothesis we are advancing; and that when viewed in this light their experiment is a contribution to psychological knowledge even more important than it is already justly recognized to be.

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DEVELOPMENT AS A PSYCHOLOGICAL REFERENCE SYSTEM[1]

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Physiological psychology distinguishes between stimulus and the responding organism (29, pp. 6 ff.). Experimental data are thus referred to and explained by the properties of organism and stimulus field. However, it was questioned at an early date whether this division was a suitable frame of reference for human psychology. Physiology, at least classical physiology, deals with abstract concepts and material facts which are not on the same level of discourse as the concrete and subjective data of psychology (8, pp. 176 ff.). Many early physiological psychologists succeeded in interpreting "isolated" forms of behavior in terms of general laws and physical entities. The psychodynamics of human experience, however, does not fit into such a system. Gestalt psychologists, e.g., abandon the old physiological models and assume dynamic brain-fields in conformity with perceived *Gestalten* (10, 13). But even these models have proved to be fairly schematic, referring to rather limited and unanalyzed forms of perception. Physiological models cannot, at any rate, *replace* psychological description and analysis.

Individual psychology has not been content with a physiological frame of reference. Such words as "ability," "factor," "trait," "quality," "disposition," etc., are not defined in terms of anatomy or physiology. At the same time, concentration on the stimulus situation, conceived of as physical surroundings, gives place to an interest in "psychical" surroundings, i.e., culture, society, and human relations. The physiological model has become concretized.

However correct may be explanations of behavior derived from individual structure and surroundings, they are nevertheless often static. Since factors, abilities, dispositions, etc., are determined from the average behavior of the individual, they often tend to "fix" him in a static scheme; deviations from the average level are then interpreted as errors, not as changes in the individual. One talks more often about unreliability in tests than about individuals altered as a result of testing. In theories of testing, especially intelligence testing, it is not uncommon to talk about re-testing individuals, as if individual behavior, in spite of one test performance, will remain the same as before. This static approach tends to exclude psychodynamic interpretations of human beings; it does not ask for the process leading up to the actual performance level.

Psychoanalysis constructs such concepts as needs, drives, instincts, etc., comparable to the concepts treated above, though referring to more primitive or irrational forms of behavior. These words, however, do not necessarily imply that psychoanalytic explanations are ultimately founded on hypostatized units, since the dynamic processes involved in so-called needs are nevertheless the focus of interpreta-

[1] The developmental method has been emphasized for a number of years by the Lund research team. Mention should be made here of Dr. U. Kragh and Mr. J. Flensburg, who are working with its theory and application. I am greatly indebted to Professor D. Krech, Professor E. Raskin, Dr. A. Salomon, and Miss L. Heims for critical suggestions.

tions; and, as a matter of fact, recent writers in psychoanalytic theory (see, e.g., 5, 26), when trying to explain the sources of motivation, pay more attention to interpersonal relations as developed in the life history than to "basic" needs and instincts. In order to explain a current neurosis, for instance, both earlier and contemporary psychoanalysis emphasizes certain former experiences, especially those in childhood. This explanation does not mean that former surroundings rather than the individual himself have determined the present neurosis; neither does it mean that any judgment can be made as to the relative importance of milieu and constitution. Experience, like behavior, is an expression of the relation between the individual and his surroundings.

We cannot say that an experience on one occasion is the cause of the coincident behavior, because they are both expressions of the same relation. It is possible, however, to say that one's experience or behavior in childhood is the cause of later behavior. Then we arrange experience in a sequence, understanding a later phase in the light of former ones. When calling an early childhood experience a cause of the current neurosis, however, one must keep in mind that this psychogenetic causality is not a "real," mechanistic one. It is, rather, a way of understanding dynamic processes, or arranging them in a scheme of life development. This approach to the individual is therefore called "developmental."

Developmental studies need the concepts of interrelations between individual and surroundings, and also the concept of surroundings, the latter especially in experimental psychology; surroundings are then the objective background irrespective of subjective experience, e.g., the test situation (or social class, income, education, etc.). Helson, for instance, in his effort to make adaptation level or the "neutral point" a basis for a theory of frames of reference, is dealing with data concerning interrelations which are defined in terms of stimuli (7).

Descriptions of development have both practical and theoretical advantages. Every science needs frames of reference or common methods of interpretation (1, 7, 9, 11, 30); in genetics, for instance, one can refer qualities to the chromatin, to gene constellations in chromosomes. Some reference systems for psychological data were mentioned above in a rather simplified way. Even the most psychological of these systems, however, easily turns into a quasi-physiological and static one, partly because it is used to replace an analysis of psychodynamic interrelations. The developmental aspect simply means that data are arranged according to their time sequence, that they are not treated as isolated structures (14) but are put into a meaningful dynamic context.

Not only does this method mean that the object of study is ontogeny. In every test situation the data may also be arranged in a developmental curve or sequence. A learning curve is, according to this point of view, nothing but a development, an adaptation to a special learning situation. Sometimes the environment remains the same all the time, sometimes it changes during the experiment; that does not matter from the point of view of arranging data. A sequence of data may have one or several dimensions. Suppose one gives a subject ten nonsense syllables and records only the number of syllables he is able to repeat on every trial. In that case one dimension of measurement has been used. But when one begins to distinguish between correct and incorrect responses, one has two dimensions. In a test constructed by Kragh and Flensberg at the Univer-

sity of Lund even more basic dimensions are used: incorrect or unorganized responses, half-correct or half-organized (emotional) responses, correct responses, and all sorts of combinations among these responses. The latter procedure in collecting data offers possibilities of structuring the developmental curve, of completing derived "longitudinal" principles of organization with several "cross-sectional" ones. Other units of structure may be preferred, or added to those mentioned above, e.g., time.

One very important advantage of developmental interpretation lies in the realm of prediction. The usual average test score is sometimes rather meaningless, especially if the standard deviation is great. How could this score predict anything about the individual's behavior? He may choose any one of many possible courses of action in the future, above or below the average. From the standard deviation one knows only that the prediction seems to be uncertain. The developmental curve can tell if his behavior changed frequently between different levels of objectivity, if his behavior formed an ordinary fatigue curve with large errors toward the end or a curve with great initial difficulties followed by great improvements, if it formed a convex or a concave curve, etc. This knowledge makes possible a prediction of the adaptation in new situations, and of behavior in well-known ones (derived from those parts of the curve where a final, stabilized level is obtained). The reaction time of an individual is perhaps below the group average. But knowing that the first part of his curve was rather labile—high values alternating with very low ones—one cannot, in spite of his low average value, recommend him for work demanding quick responses to new stimuli. His first adaptation in the morning or after a vacation might be dangerous, even if there appear no "unexpected" stimuli in his work.

Let us now give some proofs of the usefulness of the developmental method for diagnostic purposes. In an investigation of negative after-images in twins two methods of analyzing the data were used. After-images of a red square, fixated for 20 seconds at a distance of 50 cm., were projected on a grey screen, the distance of which from the eye was 100, 150, or 35 cm. The images were reported in terms of size by means of two movable rulers. Both average values from, e.g., two measurements at the same distance (e.g., 100 cm.) and the difference between these measurements were computed. The average values showed the same correlation for identical as for fraternal twins, but the differences correlated only for identical pairs. Thus after-images changed in the same way for identical twins, but often in the opposite way for fraternal twins (zero correlation); when the after-image of one identical twin grew more objective, i.e., began to agree with Emmert's law, the after-image of the other twin often was comprehended in a more objective way. Results from an investigation of the after-image duration gave even clearer results. Only when using the developmental method of calculation could we prove that identical twins are more alike than fraternal ones and get more than accidental variations in reactions (19). A new twin investigation on the Müller-Lyer illusion seems to show that a developmental interpretation can bring out correlational differences between the two sorts of twins (i.e., unveil specific adaptation processes) even in a test supposedly dealing with only well-adapted (stabilized) responses.

A mirror test was recently carried out in order, among other things, to diagnose patients in a psychiatric clinic. The subjects had to draw diagonals in squares, at first directly and then with the help of a mirror image only. The time involved in drawing five diagonals consecutively, and the number of correct diagonals, were the units of measurement; the developmental curve included ten rows of five squares each, a pause intervening after five rows.

The experiment was interpreted as an adaptation to the introduction of a radical change in the situation, an adaptation made in order to reach the original performance level (drawing without mirror). The initial difficulties (expressed by time and number of correct diagonals) could be treated in different ways by the subject, producing a rapidly falling curve or a slow one, a discontinuous or a continuous one, a rather even curve or one with steep regressions, etc. It was shown that diagnoses reached on the basis of a classification of these sequences agreed very well with psychiatrists' diagnoses, hysteroid persons, for instance, showing a special discontinuous development, *viz.*, changing between different levels of reality orientation. The sequence of data in the experiment was typical of the general adaptation pattern of the individual, and was also often typical of his ontogeny (21); the average height of the curve, on the other hand, proved to have less diagnostic significance.

In tachistoscopic experiments it is very common to make some sort of statement about the average. One investigates whether a person can apperceive an object in less than, e.g., 1/5 of a second; and if he cannot, he is perhaps classified as brain-injured. Strauss and Lehtinen (25), for instance, fix a certain border-line time for perception of objects against a patterned background. At the University of Lund an attempt has been made to arrange the experiments differently, gradually raising the exposure time in order to observe the perceptual *process*. Experiments with different materials are being carried on, e.g., on silhouettes, letters, words, colors, signs, meaningless blots, photographic pictures, and TAT-like cards (22). The apparatus used up to the present was constructed by Flensburg and allows exposure times from 1/200 of a second upwards; colored gratings, used to extinguish positive after-images, can be exposed immediately afterwards or with a pause between the picture and the grating (22). The pictures appear on an opaque glass plate, their size being about 20 cm. One case is presented below to illustrate the point.

The subject is a 55-year-old man, suffering from an organic brain disease. He was originally intelligent and a good workman. Now he has difficulty in finding words, often uses circumlocution or wrong expressions, does not fully understand what he is reading, writes and counts badly, etc. He can handle his tools correctly, however. The Head and Goldstein-Scheerer tests (the clock test, the coin test, WGS form and color test, etc.) unmask a brain-injured individual with concrete behavior, unable to differentiate the material categorically or to elevate his approach to an abstract systematic level (6). In the coin test, for instance, he cannot place the third coin of the left row on the first coin of the right row, but moves the first coin on the left side over to the analogous right coin. A tachistoscopic experiment with a silhouette of a goose and an exposure time of $1/10 + 0$ ($1/10 =$ the exposure time in seconds, $+0$ denoting that the grating follows the object immediately) [2] produces the reply: "something quick, perhaps a rat." He obviously tried to "grasp" the picture, but it was too "quick"; the experience of movement is translated into an object which moves quickly.

The next experiment began with $1/10 + 0$ seconds exposure of a factory in silhouette. Some of the results are presented in Table I (2, 23). During the whole experiment his index finger was raised to follow the outlines of the picture; he wanted to identify himself with the object, just as patients with agnosia do. The sequence of statements is rather interesting. He was able to note certain qualities (the letter L also considered to be a quality) but not the whole object until later. Thus parts were seen earlier than the entire Gestalt. The concept of a factory appeared rather early (as a *Participation*), but when even

[2] The effect of prolonging the time distance between picture and grating is (within certain limits) the same as the effect of prolonging the exposure time.

TABLE 1

RESPONSES TO A SILHOUETTE PICTURE OF A FACTORY PRESENTED TACHISTOSCOPICALLY AT GRADUALLY INCREASING EXPOSURE TIMES

Exposure time + time distance between picture and grating (seconds)	Responses
1/10 + 0	Saw something
1/10 + 1/100	Something, an L (the letter)
1/10 + 2/100	An L
1/10 + 6/100	Only the upright of the L
1/10 + 7/100	Too broad foot for an L
1/10 + 11/100	Don't know
1/10 + 14/100	Something bigger
1/5 + 17/100	A chimney, perhaps a factory
1/5 + 19/100	Two uprights?
1/2 + 2/100	Can't say
1/100 —	May be a house with a chimney
1/50 —	No
1/5 —	Some sort of factory
1/2 —	But now more roofs. I don't know what it is.
1/1 —	A factory with chimneys

1/10 + 0 means that the object was exposed for 1/10 of a second and that a grating, used to extinguish positive afterimages, followed this exposure immediately; 1/10 + 1/100 means that a pause of 1/100 of a second was inserted between the end of the exposure and the grating, etc.; 1/100 means that the object was shown for 1/100 of a second without any grating following, etc.

more roofs were detected this concept was rejected again. Parts were not added to a Gestalt, not included in the old concept; rather they were displaced by a new Gestalt, and, consequently, in the struggle between an old interpretation and a new one, blank stages appeared, stages of "I don't know," etc. (2). Generally, one may say that the subject was not able to keep several elements together without following the outlines of the figure (motoric identification). Every new element thus destroyed the old conception. The statement at 1/5 of a second was only semicorrect. Without the sequence method we would perhaps have classified his reactions as normal.

These examples, however, do not make completely clear how formal concepts are used in a developmental system. Let us assume that common psychological classifications may very well be translated into the dimensions of developmental experiments. Perhaps an individual shows a broad range of incorrect (subjective) behavior in his primary adaptation to repeated stimulation, but later only correct (objective) behavior, the quantity of behavior or the number of responses remaining the same all the time. His incorrect behavior thus becomes more reality-oriented; a transformation of "autistic" experience has occurred. If his incorrect behavior disappears, however, and is not fully replaced by correct behavior—i.e., the correct behavior does not increase as much as the incorrect responses decrease—one talks about repression, about the individual being inhibited during the period of adaptation. One also finds that these unlike behavior facades arise from fairly similar initial behavior forms which will probably appear again in new and unexpected situations. Sudden lapses into incorrect or subjective behavior (signified by an increase in time taken, or, if the time is not the unit of measurement, by an increase in the number of errors) are subsequently classified under the heading of disorganization or regression, because the subject returns to those original difficulties which had once been overcome. Type concepts may be derived from clusters of formal classifications of the curves: cumulative (progressively slowed response, lowered resistance to interference), dissociative (variable resistance to interference), stabilized (evenly maintained resistance-level), etc., as has been done in an unpub-

lished study on perception with George S. Klein at Harvard.

Discussions about reference systems might be looked upon as indicating dissatisfaction with psychological science as such, with the ambiguity involved in its concepts because of the very nature of the material. But, to use a rather outworn cliché, psychology is a science in its childhood. In many common methods of interpretation the possibilities of psychodynamic frames of reference, of purely psychological analysis, are still rejected for the benefit of borrowed *quasi* systems of explanation. Dissatisfaction is, indeed, widespread and many attempts are being made to point out new methods of interpretation, such as the important research work carried out on perception and personality (**2, 3, 9, 11, 12,** and many others). The ambiguity of psychological theory should not be accepted as a necessity, however, especially since many quite consistent ways of organizing data have already been tried (**1, 4, 7, 15, 16, 17, 18, 24, 27, 28, 30,** and many others). Perhaps the developmental aspect, emphasizing the principles of organization of growing, could add some useful trends to earlier and more recent discussions on psychological frames of reference. Anticipating the objection that this system is more descriptive than explanatory, that we nevertheless have to seek for causes outside the development, should we not ask if psychology cannot be content with its own psychogenetic causality? Or can any causal factors be sought for beyond the framework of interrelations and development?

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THE RELATION BETWEEN VISUAL AND POSTURAL DETERMINANTS OF THE PHENOMENAL VERTICAL[1]

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Fifteen years ago Koffka pointed out that perceived space could be characterized as constituting what he called a "framework" (6). Implicit in every visual perception were reference-axes of vertical and horizontal, somewhat analogous to the coordinate axes of abstract geometrical space. Objects in phenomenal space, he said, are seen to be upright or tilted or inverted only by virtue of this frame of reference. It is as if the air surrounding the objects and surfaces in our visual environment contained an invisible coordinate system with respect to which their orientation is visible.[2] Not only the perception of the position of objects but also the perception of one's own bodily posture depended on this framework. In Koffka's theory the "ego" was a part of the phenomenal field, an entity in perceived space, and consequently the maintaining of bodily equilibrium was for him essentially a perceptual process, of which the postural reflexes were simply an expression.

Koffka reached the conclusion that this phenomenal framework, this sense of the vertical which a man possesses, was determined by visual stimulation. More specifically, it depended on the main contours of the visual field such as are provided by walls, floors, and the horizon. He based this conclusion in part on some rather informal observations by Wertheimer which indicated that a visually tilted room will look upright to an observer if he continues to look at it for a long enough time. What Wertheimer did was to rotate the cone of rays entering the eyes from the room by the use of a large mirror, keeping the edges of the mirror invisible to the observer (11).

In 1938, three years after Koffka's book, I suggested, in collaboration with Hobart Mowrer, that the visual vertical and horizontal are *not* determined by visual cues but by postural stimuli, and ultimately by the force of gravity acting on the body (5). The theory was that posture was the primary capacity of an organism, and the ability to see the directions of up-down and right-left was secondary. This conclusion was based on observations which contradicted those of Wertheimer and indicated that a visually tilted room will *never* look upright to an observer. Instead, my observations suggested that a sense of the physical vertical persisted and that the artificial environment continued to look tilted by reference to it. A fairly clear issue seemed to emerge which was in need of decision or resolution. *Of the two kinds of available cues, the lines on the retinas on the one hand and the vestibular-kinaesthetic stimuli for bodily equilibrium on the other, which are decisive in the event of conflict between them?* The original observations, both Wertheimer's and

[1] This is a revision of a paper read at the Symposium on Psychological Factors in Spatial Orientation under the auspices of the Office of Naval Research at Pensacola on Oct. 31, 1950.

[2] A clockwise or counterclockwise inclination should probably be called *tilt*. The writer has recently been investigating two other types of perceived inclination which he prefers to call *slant*, (a) floorwise or ceilingwise, and (b) right-wallward or left-wallward (4). Both *tilt* and *slant* must eventually be taken into account in any complete theory of space-perception.

my own, were based on a few subjects and no precise measurements were made. The experiment needed to be repeated. Other methods of putting visual and postural cues into conflict, such as modifying the direction of gravity by a centrifugal force, also needed careful investigation.

In the 12 years since 1938 a very considerable body of facts has been accumulated which bears on this issue. Since the relation between orientation to gravity on the one hand and orientation to the visual horizon on the other is one of the classical problems of aviation medicine and aviation psychology, this research has properly been supported by the Office of Naval Research. The evidence comes mainly from a series of experiments begun by Asch and Witkin in 1942 (**1**, **2**, **12**, **13**, **14**, **15**) and from another series of experiments carried out jointly by the U. S. Naval School of Aviation Medicine and by Tulane University, under the direction of Graybiel and Mann, respectively (**8**, **9**, **10**).[3]

These results appear to be contradictory. The observers at Brooklyn College employed by Witkin made judgments of the vertical axis of space which were much influenced by the direction of the lines in the visual field and little influenced by the direction of the pull of gravity on the body. Individual differences were striking. The observers employed at Pensacola and Tulane made judgments of the vertical which were little influenced by the lines in the visual field and much influenced by the axis which the body must adopt in order to maintain equilibrium. Individual differences were not great. In the face of this disagreement, is there any way in which both sets of results can be accepted as correct?

The purpose of this paper is to suggest that the issue formulated above cannot be decided one way or the other and needs to be resolved instead. In making this suggestion, I admit having made what seems to me now a mistake. The controversy arises only if one is forced to choose between a phenomenological theory of space-perception, such as Koffka advocated, or a motor theory of space-perception such as was implied by Mowrer and me in 1938. Let us follow these opposed theories to their ultimate conclusions.

The Gestalt psychologist would like to demonstrate that perception is prior to action. Hence, he assumes that the maintaining of bodily equilibrium is a matter of perceiving one's body-position. The ego is part of a field and the postural reflexes are to be understood as forces in the field. This says, in effect, that a man must be able to sense the vertical in order to stand up. Hence, the upright posture is nothing but a secondary resultant of the spatial framework.

The behavior-theorist would prefer to believe that action is prior to perception (if it came to a choice). He assumes that equilibrium is maintained by reflex adjustments and that one's sense of the vertical is merely the conscious correlate of this process. A man has to be able to stand up, he argues, in order to sense the vertical. Hence, the spatial framework is nothing but a secondary resultant of the postural vertical.

When the theories are contrasted in this fashion the difference between them begins to sound like a terminological problem, or at most a philosophical question which no amount of evidence will settle. Is it really profitable to debate whether an animal must per-

[3] Many of the studies in this series consist of research reports by various writers distributed by the School of Aviation Medicine under the title *The Perception of the Vertical*, ONR Project Designation 143–455. *Cf.* Joint Report No. 18, *Studies in Space Perception* by C. W. Mann.

ceive the environment before he can orient to it or whether he must orient to the environment before he can perceive it? Psychologists have differed on just such questions for years, but I suspect that the argument is fruitless. Why must we suppose either that perception is prior to posture or posture prior to perception? Why not assume that they develop together, both ontogenetically and phylogenetically? Why not conceive them as reciprocally related?

The evidence indicates clearly that the apparent visual vertical, as judged by an experimental observer, is determined by *both* visual stimulation and postural (gravitational) stimulation acting jointly. The evidence is equally clear that the achieving of a vertical posture (in a tilting chair, for instance) is determined by both visual stimulation and postural stimulation acting jointly. The apparent visual vertical is the standard by which we perceive the upright or tilted quality of objects, the horizon, and the visual world around us. The postural vertical is the norm which an animal achieves by virtue of tonic muscular reactions which keep it in balance. The visual phenomena and the motor ability are closely interrelated. Why assume that one is prior to the other? Both are necessary if an animal is to see effectively and act adaptively. In all likelihood they are correlative, and neither has to be taken as the explanation of the other. We neither have to see in order to stand nor do we have to stand in order to see.

If this theoretical issue is disposed of, the question of visual cues *versus* vestibular and kinaesthetic cues appears in a new light. Spatial behavior and spatial perception depend on both modes of stimulation. The question is no longer which mode is *decisive* when they are set in conflict but simply how do they interact?

In normal upright posture in a normal environment the main lines of the retinal image are physically parallel to the direction of gravity acting on the body. When these directions are ***not*** physically parallel the situation is one of conflicting or discrepant cues. This latter situation can be produced for an upright experimental subject by rotating his retinal images, either by optical means or by actually tilting the environment which determines his retinal images. The optical method is exemplified by Wertheimer's mirror (11) and by Gibson and Mowrer's prism-glasses (5). The tilted environment method is exemplified by Witkin's tilting room (13). It can also be produced by altering the effective direction of gravity by adding a component of centrifugal force without rotating the retinal images. This method was first employed by Mach (7) and requires a human centrifuge with an *upright* visual environment. In either case the effect is to make the subject try to perceive two incompatible verticals and try to adopt two incompatible postures. In this kind of situation the subject must respond either to the retinal cue or the gravitational cues, or to a compromise between them, or first to one and then to the other.

It is important to note, at this point, that the situation of discrepant cues is *not* produced when the subject's body is tilted by his voluntarily reclining or putting his head to one side. The comparable experimental situation is that of a tilting chair with an upright visual environment. In this case, which is common in the activities of daily life, ***the lines of the retinal image remain physically parallel to the direction of the pull of gravity,*** at least approximately. On the one hand the environment (together with its projected image) and on the other hand the forces acting on the vestibular organs (and

also the skin, the joints, and the muscles) are consistent with each other, not discrepant. When, for example, a man lies on his side on the beach and looks off at the horizon of sea and sky, his retinal image has been rotated 90° out of the normal, but at the same time the pressure within his inner ear has also been rotated 90° out of the normal in the same direction. Whether he tilts his head 90° or 60° or 30° the stimulation of the retina and that of the statocyst remain consistent and may be assumed to have a sort of angular correspondence.[4]

This latter situation may be called one of co-varying cues or of reciprocal stimulation. To each degree of variation in retinal stimulation there is a corresponding degree of variation in kinaesthetic stimulation; the two are coupled together. In this situation the achieving of an upright posture is natural, easy, and accurate. A subject in control of a tilting chair under these conditions can bring himself into alignment with gravity with great precision. Moreover the tilted subject in an upright environment perceives the environment as upright and discriminates the visual vertical fairly accurately.

The above result is in striking contrast with that of the first situation, the *upright* subject in a *tilted* environment. The subject may or may not perceive the tilt of the environment, and he can discriminate the visual vertical only with a considerable error and with much variability (13). In both situations there has been a rotation of the retinal image out of the normal, but in the second there is no reciprocal change in the accompanying postural stimuli. The two modes are still coupled but the stimulus-variables do not correspond. The rotation of the retinal image is not compensated for by a corresponding angular shift in the proprioceptive complex.

The theory which emerges from these considerations is something like this. In the case of reciprocal visual-proprioceptive stimulation, the coupled variables combine to form an *invariant* resultant which is in correspondence with the objective direction of gravity and which provides the stimulus for a univocal impression of the vertical. This is why the ordinary visual scene continues to look upright when one inclines his head and thereby rotates his retinal image.[5] In the case of discrepant visual-proprioceptive stimulation, or conflicting cues, the coupled variables do not yield an invariant. Their values do not correspond in the regular fashion and the resulting perception is ambiguous or equivocal. The subject is instigated to perceive two different verticals at the same time. The organism is forced to choose between them. Accordingly it is not surprising that the resulting perception is unstable and that it differs from one subject to another, as Witkin has discovered. Determinants of perception other than stimulation are free to function—attitudes, expectations, and habits characteristic of the person observing in the situation.

Here, in truth, is the case where the perception can be said to be determined by cues instead of by stimulus-variables.

[4] This formulation, it is true, neglects the slight counter-rolling of the eyes when the head is tilted. But this adjustment may be conceived as simply a part of the compensatory perceptual-motor mechanism being described, which complicates but does not invalidate the theory.

[5] The hypothesis of an "invariant of stimulation" in a number of perceptual situations was the insight which enabled Koffka to comprehend the problems of perceptual constancy more clearly than any other investigator. This hypothesis may be adopted without accepting his theory of field-forces in perception. The above application of the concept of an invariant is quite different from that of Koffka (6, pp. 215–218), but the conception itself is his.

The observer must search for clues to, or indicators of, the direction of gravity. His perception is objectively correct only to the extent that reliable clues are discovered, and consistently correct only to the extent that they are verified and learned.

The explanation of space-perception in terms of cues is roundabout and requires more hypotheses than an explanation in terms of stimulus-variables (3). Nevertheless when stimuli are contradictory they can only function as cues or clues. Let us apply the language of clues to the case of the subject seated upright in a tilted room. His retinal image of the environment taken by itself "tells him" that his head and body are inclined away from the vertical (this being the ordinary and almost invariable cause of a tilted retinal image). But his inner ear, his muscles, and the seat of his pants "tell him" that his body is *not* inclined from the vertical. Which sense is he to believe? If he believes his eyes, he should attempt to align his head with the room and, when asked to rotate the adjustable stick into a vertical position, he should set it nearly or completely parallel to the side walls. The room would appear nearly or wholly upright. On the other hand, if he believes his body-sense but not his eyes, he should sit askew of the room and he should set the stick in line with the main axis of his body. The room would appear to be strongly tilted relative to a larger space outside the room. If he cannot decide between his senses, he may begin to make inferences—unconscious inferences according to Helmholtz. If he has been told to set the stick to the *true* vertical, for instance, he may reflect upon the probability that gravity and balance are "true" and decide to put trust in the body cues. If he scans the visual evidence, however, the retinal lines argue forcibly that his head must be upright when he holds it aligned with the room.

The reason why Witkin's observers tended to see the vertical in alignment with the room whereas the Pensacola-Tulane observers tended to see the vertical in alignment with gravity may not be hard to find after all. If differences in the attitude of the subjects can affect the judgments, this fact may be sufficient to supply the explanation. The very meaning of the term "vertical" can be ambiguous in the situation described, and one group of subjects may have understood by it the apparent vertical while the other group understood by it the objective or physically correct vertical.

The writer has recently obtained evidence to show that, for a closely allied type of space-perception, subjects can distinguish between the "optical" slant of a surface and the "geographical" slant of the same surface when the line of sight is not horizontal and straight ahead but turned (4). The same distinction may prove to be valid for experiments on tilt. When the head is rotated around the horizontal line of sight, my own observations suggest that subjects will be able to perceive an optical vertical and a gravitational vertical independently and correctly, if asked to do so. Both are determined by stimulation, the former by visual stimulation alone, the latter by a visual-proprioceptive invariant. Both may prove to be stable and consistent in this situation.

According to the theory outlined, ambiguous, equivocal, or unstable perceptions of the gravitational vertical occur when the modes of stimulation conflict. More precisely, they are the result of the absence in stimulation of a visual-proprioceptive invariant. Along with the unstable perception often goes an unstable posture, i.e., some degree of disequilibrium. It is important to study

this situation, but it is even more important to understand the basic stimulus-situation of co-varying modes. The reciprocity of vision and proprioception in everyday spatial behavior may prove to be not only the key to the problem of upright posture in relation to the visual vertical but also the key to the problem of geographical orientation. In the most general sense, the covariation of vision and proprioception is probably at the heart of the problems of locomotor behavior, pursuit tasks, and many other motor skills.

It is important to realize that the individual differences which Witkin found to be so striking in the situation of conflicting stimulus-modes (12) are, in all probability, a function of that situation. He has not demonstrated that individuals differ in the basic ways in which they perceive space, but only that they differ in their choice of alternatives when several possibilities for perception are open, i.e., when the process of spatial perception is ambiguous. When the stimulus conditions are indeterminate, the outcome will be influenced by attitudes, motives, and even by social background, sex, and temperament.

On the other hand, the situation of conflicting cues is not unimportant, nor is it wholly unrepresentative of spatial behavior. Equilibrium and orientation in an airplane, and other complex forms of spatial behavior mediated by instruments, probably involve some degree of conflict or discrepancy among cues. The practical problem here is that of learning to use the reliable cues and to neglect the unreliable and irrelevant ones.

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THE DILEMMA OF FEAR AS A MOTIVATING FORCE

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Learning theorists, faced with the problem of explaining avoidance training and animal fixations, have postulated that an animal may respond in order to reduce fear. Freud made a similar postulate in the area of human behavior. In this paper I should like to show that the postulate leads to a dilemma.

The Original Dilemma and Its Proposed Solution

Avoidance training is one type of instrumental conditioning in which an animal, rewarded for the correct response or punished for the incorrect, soon limits its behavior to the correct one. Thorndike's problem-box typifies instrumental conditioning of the kind which Hilgard and Marquis (2) designate as *reward training*.

A modification of this is *secondary reward training*, as when animals work for tokens to be exchanged for food. Because both Pavlov's classical conditioning and Thorndike's instrumental conditioning suggested that reward reinforces a response, an attempt was made to fit secondary reward training into this formulation. This meant that obtaining the token was rewarding to the animal. Since the token was originally a neutral object, the concept of secondary, acquired, or learnable rewards or drives developed (4).

While in reward training the important reinforcement is reduction of some physiological tension such as hunger or thirst, in *escape training* it is reduction of tension aroused by the painful goading stimulus, usually electric shock:

> ". . . Escape and reward . . . are similar in their dynamic action as reinforcement for learning. . . . Indeed, reward might in some cases be considered an escape from the persisting stimuli which characterize the drive. Muenzinger and Fletcher (1936) have reported an experimental comparison of hunger-food-tension and shock-escape-tension as reinforcement for the learning of a visual discrimination by rats. The effectiveness of each may be interpreted as an escape from tension present in the learning situation; in one case hunger, in the other, pain. Shaffer (1936) has also emphasized tension reduction as the essential characteristic of problem solution (2, p. 83)."

Out of these escape experiments, in which the animal briefly experiences shock, arose *avoidance training* in which the animal learns to avoid the shock altogether. This presented a dilemma for the reinforcement theorists: Can there be "an escape from the persisting stimuli which characterize the drive" when "the learned reaction prevents the appearance of a noxious stimulus" (2, p. 58)? Mowrer has also discussed this paradox (5, p. 86).

When extinction phenomena were considered, the same dilemma arose. After noting that "if the conditioned stimulus is repeatedly presented without its usual reinforcement, the conditioned response undergoes a progressive decrement and finally fails to occur" (2, p. 104), and that "omission of reinforcement leads to the weakening of a conditioned response" (2, p. 104), Hilgard and Marquis state that there are "instances in which the expected decrement does not occur" (2, p. 120), and offer an avoidance experiment as an example. Maier's frustration experiments (3) are also instances in which the expected decrement does not occur.

Secondary reward training suggested an answer to this dilemma. Hilgard and Marquis refer to the "reinforcing action of a sub-goal" (2, p. 64) in secondary reward training, and suggest that "an explanation of the reinforcing effect of sub-goals will . . . go far toward an explanation of avoidance learning" (2, p. 65):

"Can an explanation in terms of r_g's [fractional goal reactions] be applied to avoidance learning? An affirmative answer is intimated by the experiment of N. E. Miller. . . . This heightened tension is relieved by the avoidance behavior. . . . Since the animal succeeds in reducing tension, the situation has become dynamically that of escape learning. . . . Hence in later trials, although the shock is not received, the avoidance behavior continues to be reinforced, because escape inevitably reduces the conflict produced by approach to the shock point. . . . A similar explanation has been proposed by Mowrer (1939) who identifies the anticipatory r_g's with fear or anxiety" (2, pp. 245–246).

THE NEW DILEMMA

I should like to illustrate the dilemma by discussing Maier's frustration experiments. Ordinarily one can train the rat on the Lashley apparatus to select either of the two windows by selectively locking the negative one. This is simple reward training. However, if one locks the windows randomly, the animal soon gives up trial-and-error jumping and leaps consistently to one of the windows. After several days of this the animal may continue leaping at that window even if given the orderly punishment of the symbol-reward problem, and even if the alternative window is opened, exposing food.

In order to reconcile this behavior with reinforcement theory, Mowrer postulated that the animal now jumps in order to reduce fear, and that reduction in this tension reinforces the response: ". . . we are dealing with fear as the dominant motive . . . the animals are trying to get rid of . . . their fear . . ." (5, p. 357).

In other words, the rat jumps to the habitual window, permanently locked, rather than to the open one, because the animal is not concerned about eventual consequences but only about immediate reduction of fear, which requires "merely that the rat *get off the jumping stand*" (5, p. 357). Were the rat concerned with both goals, reducing fear and avoiding punishment, it could achieve both by leaping to the open window. The fear-reduction theory thus leads to the postulate that the animal's sole concern is with an immediate reduction in fear. And that is the dilemma.

If the animal is no longer concerned about the locked window, whence the fear? If the fixated rat is indifferent to the punitive consequences of its leap, it has no basis for experiencing fear at the prospect of punishment. Here is an animal no longer concerned with punishment, yet experiencing the fear which arises from anticipation of punishment.

There is another aspect to the dilemma. Let us assume that the animal is still fearful of the locked window. There is still no logical basis for fear; for, with an open window available, the animal need not jump at a locked one. Yet it continues to fear punishment which need not occur.

A HUMAN ANALOGY

A man is condemned to die on the morrow. His goal is to avoid death, to live. However, he sees no way out and must resign himself to the inevitable. He feels afraid of the impending execution and takes advantage of whiskey to alleviate his fear.

We might now conclude that because his behavior is oriented toward relieving immediate tension, his goal is no longer

one of avoiding death, so that there is no fear for him to relieve. By this logic the natural situation of a doomed man trying to make his last hours tolerable could not exist, for if his concern is with his distress, and not with his doom, he has nothing to feel distressed about.

The key to this contradiction is the fact that the man is faced with an insoluble problem. Fixation is persistent, unadaptive behavior when a better response is possible (3, p. 33). Suppose that the captive discovers a window through which he can escape. If he takes advantage of this "open window," then he had two goals, and was concerned both with immediate tension reduction and with eventual escape.

If, like the fixated animal, he looks at the open window without taking it, then he apparently is no longer oriented toward avoiding death. We might now ask, "If he is no longer concerned about death, why does he feel any dread which he must reduce with whiskey?"

He should feel none: First, because having dropped the goal of avoiding execution, he should no longer fear it; secondly, because even if he has retained the goal, there is an open window, and the execution need not occur. But if his behavior is similar to that of a fixated animal, he peers at the open window, fails to take it, and continues in the fear-reducing response.

CONCLUSION

The fear-reduction theory thus leads to a double dilemma: First, an animal motivated only to reduce immediate tension still experiences tension which arises from anticipation of more distant consequences. Secondly, an animal which can easily avoid punishment still fears the punishment and, in Maier's experiments, makes the punished response when it need not do so.

In brief, if we use the fear-reduction theory to explain why a punished response persists, we must then ask why the fear persists.

Freud clearly recognized that his assumption of anxiety reduction as the goal of neurotic behavior led to this dilemma, that the neurotic individual experiences anxiety when there is nothing to feel anxious about:

> . . . In their response to danger so many people remain infantile, continuing to react with anxiety to situations which should have long ceased to evoke it; to dispute this would be to deny the very fact of neurosis, for it is exactly such persons whom we call neurotics. But how does this situation come about? Why are not all neuroses merely episodes in the individual's development which become a closed chapter when the next stage of development is reached? Whence comes the element of permanency in these reactions to danger? Whence springs the preference over all other affects which the affect of anxiety seems to enjoy in alone evoking reactions which we distinguish from others as abnormal and which in their inexpediency obstruct the stream of life? In other words, we find ourselves abruptly confronted once again by the oft-expressed riddle: What is the source of neurosis, what is its ultimate, its specific, underlying principle? After decades of analytic effort this problem rises up before us as untouched as at the beginning (1, pp. 91–92).

The postulate of reinforcement through anxiety- or fear-reduction, as a mechanism underlying fixated behavior in animal and man, leads from the dilemma of autonomous behavior to the dilemma of autonomous fear. In our attempt to cope with this paradox of self-perpetuating, self-defeating behavior, which Mowrer has called the crucial challenge to personality and learning theory, we stand today where Freud stood, with the problem unsolved.

SUMMARY

Avoidance training presents a dilemma to learning theorists. What is the source of reinforcement if the animal avoids the noxious stimulus altogether?

Fear-reduction is one answer to this dilemma. This answer, however, when applied to the fixations of Maier's frustration experiments leads again to a similar dilemma: What is the source of the fear? If the animal, leaping at a locked window when an open one is available, is no longer oriented toward avoiding punishment but only toward reducing fear, it has no basis for experiencing fear at the prospect of punishment. Moreover, in animal fixation as well as in human neurosis there is no actual threat. The fear-reduction theory thus leads from the dilemma of autonomous behavior to the dilemma of autonomous fear.

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LEARNING THEORY AND CULTURE[1]

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INTRODUCTION

In recent years certain psychologists have emphasized the importance of analyzing personality in terms of both learning theory and culture theory. One systematic attempt to make such an analysis can be found in *Personality and Psychotherapy* by Dollard and Miller (1). To carry out an analysis of this sort successfully requires at least that the central learning theory concepts be made explicit and the concept of culture be given similar treatment. It follows, therefore, that if either of these two systems is not properly explicated, then the problem of explaining personality by these concepts cannot be said to have been accomplished. An examination of *Personality and Psychotherapy* indicates that the authors have devoted their attention almost exclusively to the task of formulating and using the central notions of learning theory, and have paid scant attention to the problem of clarifying the concept of culture. They do make rather frequent use of the word "culture" but do not at any point make a logical analysis of this term such that a clear and consistent interpretation can be made of the statements in which it is used.

In some instances "culture" and "society" are seemingly treated as synonyms. There is nothing inherently wrong in treating "society" and "culture" as synonymous terms, although most sociologists and some anthropologists insist that a clear distinction be made between the two. In other parts of their book Dollard and Miller evidently intend that the term "culture" be used to designate the ways that people, living in a society, solve their problems insofar as these ways of solving problems have been handed down from one generation to another. When they state that "Some of the elements of child-training procedures are undoubtedly thousands of years old" (1, p. 127), they are probably thinking of these methods of rearing children as items of culture. In keeping with this usage they state, "One important function of culture, as Ford has pointed out, is that it represents a storehouse of solutions to recurrent problems" (1, p. 20). This statement is as close as they come to a definition of the term "culture." It is not a definition; it merely indicates what one important function of culture is. Another important function of culture, according to their views on the matter, is that it *creates* problems because it contains many strains and inconsistencies.

[1] The authors wish to express their appreciation to the Carnegie Corporation of New York whose grant to Northwestern University made possible their participation in developing an elementary course and in helping to conduct a seminar, both sponsored jointly by the departments of anthropology, psychology, and sociology. The authors also wish to express their especial appreciation to Professor Kimball Young and Dr. David Olmsted for their constructive criticism of this paper.

[2] The opinions and assertions contained herein are the private ones of the writers and are not to be construed as official or reflecting the views of the Navy Department or the naval service as a whole.

As we go through this work we find that culture is truly a surprising phenomenon. It is not only a "storehouse of solutions to problems," and a storehouse of problems requiring solution, but it also "acts as a selective agent," and sometimes culture is viewed as an "inclined plane" up which children are forced.

This same looseness of expression about culture is evident in their earlier work, *Social Learning and Imitation* (2). They say, ". . . culture, as conceived by social scientists, is a statement of the design of the human maze, of the type of reward involved, and of what responses are to be rewarded. It is in this sense a recipe for learning" (2, p. 5). It seems very unlikely that social scientists will ever find it convenient to conceive of culture as any kind of a statement, whether it is a statement of the "human maze" or not. If such a conception, i.e., that culture is a statement of the design of the human maze, were to be found useful, then this statement would have truth value (be either true or false). If culture is a recipe for learning, such a recipe would have no truth value. A recipe consists of a set of commands or imperatives which are neither true nor false. It would thus seem inconvenient for culture to be a statement both with and without truth value. If their work were intended to be a literary product, the use of figurative language would be in order and no one would protest against using one of the key concepts in a variety of senses. However, scientific work requires at least that the central concepts employed in an analysis be determinate and consistent. Only in this way can we avoid the construction of nonsense.

BASIC ASSUMPTIONS ABOUT CULTURE

There are a number of fundamental assumptions about the nature of culture which are shared by most anthropologists and sociologists. Any adequate explication of the concept of culture certainly ought to take these assumptions into account. G. P. Murdock (4) [3] has listed seven of these fundamental assumptions. We shall consider in some detail each of these assumptions about the nature of culture. Our purpose in examining these assumptions is to arrive at the minimal characteristics which anything must have in order to be properly termed "an item of culture." When we have finished examining these basic assumptions we shall then be in a position to give a syntactically determinate definition of "culture."

The first assumption is: "*Culture is learned.* Culture is not instinctive, or innate, or transmitted biologically, but is composed of habits, i.e., learned tendencies to react, acquired by each individual through his own life experience after birth" (4, p. 364). This assumption, according to Murdock, is accepted "by all anthropologists outside of the totalitarian states."

Culture, then, consists of learned responses, but we must be careful not to draw the conclusion that all learned responses are cultural. As we shall see, only some learned responses are cultural.

The second fundamental assumption is: "*Culture is inculcated.* All animals are capable of learning, but man alone seems able, in any considerable measure, to pass on his acquired habits to his offspring" (4, pp. 364–5). This assumption, taken with the preceding assumption, implies that for anything to be properly termed "an item of culture" it must be, first of all, a learned response, and, secondly, that it must be passed on from parent to offspring. As a matter of fact, there is no reason to restrict the

[3] The authors recognize that there are other formulations of the important assumptions concerning culture, but as Murdock states about his assumptions, "These are not claimed to be original, since many of them are shared by all social scientists, and all of them by many" (4, p. 95).

meaning of the term "culture" so that it applies only to those things that children learn from their parents. As Murdock undoubtedly would agree, the parent to child transmission is not the only source of cultural learning. Anything that one individual learns from any other individual will be considered an item of culture, and everything that any given person learns from others will constitute that person's culture.

The word "inculcate" was used in the statement of the second assumption about culture. This word ordinarily means "to teach or instill by frequent repetitions or admonitions." The use of this word may suggest that for anything to be an item of culture it must be taught, perhaps taught in a formal way as many things are taught in school. It is true that some items of culture are learned in formal teaching situations. However, some of the most important items which are learned from other persons are not "taught." Whatever one person learns from another, whether in school, in the home, in a bar, etc., is part of his culture.

In order to learn anything it usually is necessary to make a number of attempts or trials. Some responses, however, are learned in a single trial and do not require many repetitions or admonitions. Sociologists and anthropologists may be most interested in those learned responses which have been instilled, i.e., have become a permanent part of the response repertoire. Such instilled responses have usually been learned in situations in which admonitions from others play an important part in the learning process.

In summary, whether a response learned from others is transitory or is a permanent part of the response repertoire, whether it was learned in a single trial or many trials, whether it was "taught," or "just picked up somehow," it is an item of culture.

The third basic assumption is: "*Culture is social.* Habits of the cultural order are not only inculcated and thus transmitted over time; they are also social, that is, shared by human beings living in organized aggregates or societies and kept relatively uniform by social pressure. They are, in short, group habits. The habits which the members of a social group share with one another constitute the culture of that group" (4, p. 365).

From the first two assumptions we have found that for anything to be properly termed an "item of culture" it must be a learned response and a response learned by one individual from at least one other individual. It would seem, therefore, that, because at least two persons are involved, the learning situation is necessarily social in character. However, in his explanation of the assumption "Culture is social," Murdock has made an unnecessarily restrictive stipulation. By "social" he means that culture is "shared by human beings living in organized aggregates or societies and kept relatively uniform by social pressure." It is undoubtedly true that culture is generally kept relatively uniform by social pressure. Social pressure, however, may, and sometimes does, fail to keep culture relatively uniform. But an item of culture would not lose its status as an item of culture because of the failure of social pressure. In other words, uniformity is not a necessary condition for a learned response to be considered an item of culture.

There is one further matter which must be considered. Murdock states that "The habits which the members of a social group share with one another constitute the culture of that group." This statement is somewhat ambiguous. If he means that the culture of a group consists of only those habits (responses) shared by all members of the group, this excludes the specialized responses which

may be shared by less than all members of the group. In dealing with the second assumption, we noticed that the more permanent responses are perhaps of more interest to an anthropologist or sociologist than the extremely ephemeral ones. In examining this third assumption, it is probably also true that responses common to all members of a group are of more interest than responses common to less than all members of the group. But we are attempting to discover the minimum conditions that must be satisfied for anything to be considered an item of culture. Therefore, any response learned by at least one individual from some other individual would be an item of culture. This entails that the situation is social in character.

The fourth assumption is: "*Culture is ideational.*" Murdock notes that linguistic and symbolic behavior is learned and therefore the fourth assumption may be subsumed under the first assumption. What we have said about the first assumption is also true of the fourth assumption.

The fifth assumption is: "*Culture is gratifying.* Culture always, and necessarily, satisfies basic biological needs and secondary needs derived therefrom" (4, p. 366). If learning theory of the reinforcement variety is correct, no response will be learned unless it is drive reducing, i.e., gratifying. A response, however, that has been learned may not always continue to be drive reducing and hence, according to reinforcement theory, should extinguish in time. Murdock makes this same point in a footnote. He says that the only exceptions to the assumption that culture is gratifying "are partial and temporary ones, with respect to elements of culture in the process of dying out or being supplanted." All culture, then, is not gratifying, even assuming that reinforcement learning theory is correct. Therefore, we need not take gratification as a necessary minimal condition to be satisfied for anything to be properly termed an "item of culture." Reinforcement theorists have studied the extinction of responses under controlled laboratory conditions and can predict, with some degree of success, how long a nonreinforced response will persist under these conditions. There are, however, very few data available which would indicate how long a nonreinforced response might persist in the variety of social situations that anthropologists or sociologists study. Given the assumptions of reinforcement theory, it is analytically true that a response is gratifying when learned. But any response, once learned, may be extinguished, and in the time between the learning of a response and its extinction that response may be nongratifying and still exist as an element of culture.

The sixth assumption is: "*Culture is adaptive.* Culture changes; and the process of change appears to be an adaptive one, comparable to evolution in the organic realm but of a different order" (4, p. 367). A human infant is an extremely helpless and vulnerable creature. His innate, or biologically transmitted, responses are not sufficient to keep him alive. If an infant is to survive, he must learn a host of complicated responses from other individuals. In this sense, then, the culture of any given individual may be said to be adaptive. However, a person may learn responses from other persons which lead directly to his destruction. The culture of a group of persons may, and sometimes does, lead to the destruction of that group. But the mere fact that millions of human beings are living today indicates that some cultures have considerable survival value. We may conclude, therefore, that, although the responses that are learned from others function in general to promote the survival of individuals and groups, this is not necessarily the

case. The minimal conditions that must be satisfied if anything is to be termed an "item of culture" do not require that it be adaptive in character.

The seventh assumption is: "*Culture is integrative.* As one product of the adaptive process, the elements of a given culture tend to form a consistent and integrated whole" (4, p. 368). Whatever Murdock may mean by "integration," he makes it quite clear that culture only *tends* to be integrative and is, therefore, not necessarily integrated at any given moment in time. It can be concluded, therefore, that just as culture is not necessarily adaptive, it also is not necessarily integrated. This is, like so many other things, a matter of degree. A culture may be or become more integrated or less integrated. The invention of new ways of responding and their introduction into any given society may or may not conflict with previously established ways of behaving. If they do not conflict and are incorporated as part of societal behavior, we can speak of an increment to the existing culture. In terms of a long historical perspective, we can see that culture has been cumulative in character. More new items and groups of items have been added than lost. We should not forget that some culture items have been lost and some whole culture systems have disappeared.

In some extreme cases the learning of a new way of responding entails the extinction of an old way. Such culture conflicts are often violent and extremely painful to the participants. A society undergoing such conflicts can scarcely be said to have an integrated culture. But if the acceptance of the new ways means the extinction of the old, then in time (other things being equal) we can expect a new integration.

We have now completed our examination of the seven basic assumptions about the nature of culture as presented by Murdock. Our purpose in examining these assumptions was to arrive at the minimal characteristics which anything must have in order to be properly termed "culture." We chose to examine Murdock's statements because he presents, relatively succinctly, and in one-two-three order, a set of ideas about culture which are widely shared by anthropologists and sociologists.

We have found that for anything to be an item of culture it must be, first, a learned response, and, second, it must be a response learned by at least one individual from at least one other individual. Two persons involved in such a learning situation justify one in viewing the situation as social in character. The learned response need not necessarily be permanent as opposed to transitory, neither must it be gratifying as opposed to nongratifying, adaptive as opposed to nonadaptive, or integrative as opposed to non-integrative.

PROPOSED DEFINITION OF "CULTURE"

Statement of definition. The reason we have been interested in specifying the minimal conditions that anything must meet to be termed "culture" is twofold: (1) we want to formulate a syntactically determinate definition of the term "culture," and (2) we wish to frame a definition which will have maximal utility in facilitating cross-disciplinary cooperation between psychology, anthropology, and sociology. If a definition is syntactically determinate it will have a structure articulated in such a manner that the logical consequences of its employment may be determined. A definition is essentially a stipulation as to how a certain term is to be employed. Because a definition is stipulational it may not be said to be either true or false. In this respect a definition is unlike a proposition, for a proposition has truth value. The criterion by which a definition is judged is that of utility. The utility of a definition depends upon the

way it functions to facilitate inquiry. We believe that the following definition by one of the authors, Moore (3), will facilitate the cross-disciplinary cooperation of psychologists, anthropologists, and sociologists.

'Culture' = '$\hat{y}$ (x learns y from z and $x \neq z$)' by df. (to be read 'the class of values of the variable y such that x learns y from z, and x is not identical with z').

Notes: (a) The variable x in the propositional function 'x learns y from z and x is not identical with z' takes as values any hominid individual.

(b) The variable z in the propositional function 'x learns y from z and x is not identical with z' takes as values any hominid individual other than the one which is taken as a value of the variable x in any given instance. The fact that in any given instance the values of the variables x and z must not be the same is indicated in the propositional function by '$x \neq z$.'

This definition contains terms which may be unfamiliar to some readers. Our explanation of this definition will not presuppose that the reader is acquainted with symbolic logic. In order to explain certain ideas that have been worked out in symbolic logic, without becoming unnecessarily technical, it will be most convenient to use examples which may seem unduly trivial.

Let us begin by examining the proposition "John Jones learns to tie a square knot from Bill Smith." The set of responses that Jones has learned would constitute an item or items of culture. Let us further assume that John Jones has learned many other things from Bill Smith. Whatever John Jones has learned from Bill Smith constitutes *part* of the culture of John Jones. Of course it could be that everything Jones has learned, from someone other than himself, has been learned from Bill Smith. In this case, obviously, all of John Jones' culture was learned from Bill Smith.

Logical variable. At this point it is convenient to introduce the notion of a logical variable. In the expression "John Jones learns y from Bill Smith" the variable "y" may be thought of as a symbol whose function is to hold a place open which can be filled in by anyone of a number of values.[4] The whole expression "John Jones learns y from Bill Smith" is a propositional function. This particular propositional function determines a class. The class that is determined consists of all those values of the variable y which, when substituted for y, yield a true proposition. Let us suppose, in this example, that there e five such values: (1) "to tie a square knot," (2) "to row a boat," (3) "to build a canoe," (4) "to drive a nail," (5) " to pitch a tent." These five behaviors that John Jones has learned from Bill Smith constitute the members of the class determined by the propositional function "John Jones learns y from Bill Smith." Each one of these values, when substituted for y, yields a true proposition. In any investigation of life situations, however, it would be an empirical matter to determine what someone either has or has not learned from someone else.

Let us now turn our attention to the variable z. The propositional function to be considered is now "John Jones learns y from z." The variable z takes

[4] Strictly speaking it is the name of a value that is substituted for a variable. The values themselves here, and throughout the essay would, of course, be extra-linguistic entities (or on another language level) and, hence, could not replace a variable in these *expressions*. The present terminology is employed to avoid awkward circumlocution.

as values any hominid. Since John Jones is a hominid, his name might be substituted for z, and this would entail our accepting the proposition, "John Jones learns to tie a square knot from John Jones." To say that John Jones learns something from John Jones is an eliptical way of saying "John Jones has invented something." It was previously pointed out that anthropologists, according to our interpretation of their views, do not wish to consider anything to be an item of culture unless: (1) it is a learned response, and (2) it is learned from someone else. To avoid this difficulty, i.e., that John Jones may be a value of the variable z, a stipulation was made to prevent this from happening. The stipulation is "$x \neq z$," which means that x and z may never be the same individual in any given substitution instance.

The propositional function that we have just been considering "John Jones learns y from z" may now be amended to read "John Jones learns y from z, and there is no z such that John Jones is identical with z." This amended propositional function determines a class, and the membership of the class consists of everything John Jones learns from anyone other than himself. This, then, is the culture of Jones. The culture of John Jones at any given time consists of everything that Jones has learned from anyone else and that has not been extinguished. If it were the case that John Jones had not learned any response from any other person, then he would have no culture, i.e., would be a feral man. A feral man is one who, although capable of learning, has not learned anything from anyone else. The concept of the feral man is an important one whether or not such an individual has ever existed; it is desirable to be able to state precisely what could be meant by saying that someone lacked culture.

The remaining variable to be considered is x. The employment of x allows us to formulate the propositional function "x learned y from z and x z." This propositional function also determines a class. The class thus determined consists of everything anyone has ever learned from anyone else. Since x and z can take as values only hominids, the class determined by this propositional function is the totality of hominid culture. Of course, no given anthropologist or sociologist could study this whole class of events. An anthropologist usually elects to study the culture of some particular set of hominids at some particular time and/or to make comparisons between cultures at various times.

Concept. Throughout this paper we have dealt with the concept of culture. The word "concept" is one which is relatively ambiguous. By some authorities the word "concept" and the word "conception" are treated as synonymous terms. Very loosely speaking, the concept of culture would be someone's idea about culture. This usage tends to confuse two related but quite different matters. The first is the psychological status of an analytic device, and the second is the logical status of an analytic device. In this paper the word "concept" will be treated as synonymous with "propositional function." The concept of culture is, then, "(x learns y from z and $x \neq z$)."

Abstraction operator. There remains but one matter to consider before the exposition of the definition of the term "culture" is complete. This concerns the capped letter "y," i.e., "$\hat{y}$," which occurs outside the brackets and to the left of the propositional function "(x learned y from z and $x \neq z$)." The capped y is technically known as an "abstraction operator," and it functions in the following way: Through the addition of the capped letter "y" to the propositional function, the class deter-

mined by the propositional function is named. This capped letter may be thought of as a prefix to the propositional function which may be read "the class of values of the variable y such that . . ." "$\hat{y}$ (x learned y from z and $x \neq z$)" is the name of a class and the class so named is culture. Any phenomenon which is a value of the variable y in the propositional function is to be called an item or element of culture, and all such phenomena constitute the class culture.

Advantages of definition. The definition which has been presented has certain advantages which will now be considered. First of all, the logical consequences of its employment can easily be determined. One can tell what characteristics anything must have in order to be called an "item of culture." In short, it is syntactically determinate. Second, it focuses attention upon just those things which cultural anthropologists working in the field usually study. Third, it is parsimonious.

In any given scientific discipline there are certain terms which, for purposes of convenience at least, must be defined. All of the terms used within any given discipline, however, cannot be defined by means of terms within that discipline. If they were, this would amount to vicious circularity. Clearly, some terms must be primitive, i.e., in this case, remain undefined within anthropology or sociology. It is important to know what terms are to be taken as primitives and also to choose if possible as primitives those terms which are carefully defined within some other discipline. The definition of "culture" that has just been presented involves a number of terms which are primitive with respect to cultural anthropology and sociology. Some of these primitive terms are drawn from the field of symbolic logic, and have been carefully explicated in that field. The term "hominid" is drawn from biology and physical anthropology and has also received careful explication in those disciplines. Still another primitive expression is "learns from." Psychologists, of course, are the authorities in the field of learning theory and it is to psychology that we must now turn in order to discover what the expression "learns from" means.

Before considering "learns from" it may be wise to treat "learns." Even a cursory examination of the relevant psychological literature indicates that there are a number of learning theories. The meaning of the term "learns" differs in each of these theoretical systems. It must be admitted, therefore, that if the term "learns" is not syntactically determinate in psychology, then the definition of "culture" which has been presented in this paper is also not syntactically determinate. But if, for instance, one chooses to follow the usage of the term "learns" as it is employed in reinforcement theory, the definition of "culture" may be considered to be syntactically determinate. In our opinion reinforcement theory has been more carefully worked out than any of the competing theories.

The expression "learns from," however, is one that has not received systematic treatment, *per se*, in learning theory. Miller and Dollard—reinforcement theorists—have worked out a concept of "matched dependent" behavior which may serve as an explication of "from" in the expression "learns from." If John Jones learns to tie a square knot from Bill Smith, then Jones' performance is *dependent* upon certain actions of Bill Smith's and certain of his responses are like those of Bill Smith, i.e., are *matched* to those of Bill Smith. Another way of stating this is to say that in a specified situation certain of Bill Smith's actions are cues for certain of Jones' actions, and that certain of their actions are matched.

Our usage of the expression "learns from" includes both what Miller and Dollard call "matched dependent" behavior and also what they call "copying." They consider matched dependent behavior and copying as two quite different learning activities, which seems to be the case. But they make the distinction between the two on the basis that the copier must be aware that his copying falls within the band of tolerance which will allow it to count as a match for the model. According to them, in matched dependent behavior this kind of awareness is not present. In our opinion, the distinction between copying and matched dependent behavior, although a necessary one, should not be based on the notion of awareness, but rather upon the amount and kind of interaction which obtains.[5]

SUMMARY AND CONCLUSION

It has been pointed out that if Dollard and Miller were successfully to treat personality in terms of learning theory and culture theory, they must at least make the concept of culture syntactically determinate. This was not done. In this paper we have presented the concept of culture in such a manner that the logical consequences of its employment can be determined. The concept of culture presented here was formulated to take into account certain of the basic assumptions about the nature of culture which are held by cultural anthropologists and sociologists. This definition of the term "culture" is also one which makes clear certain of the interconnections between learning theory and culture theory. Undoubtedly many definitions of the term "culture" which have been devised by anthropologists and sociologists are useful for certain purposes. There is no such thing as *the* definition of "culture." A definition is essentially a stipulation, and hence any number of stipulations may be made if it is profitable to do so. What is not permissible, within any given inquiry, is to violate one's own stipulation without making this change in policy explicit. It should be noted also that if a definition is not determinate, then it will be impossible to discover whether the term defined is being employed as stipulated or not. The definition presented here should be of use to anyone interested, as Dollard and Miller were, in relating modern learning theory to modern culture theory.

[5] This topic will be treated in a subsequent paper by the authors and Kimball Young.

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A COGNITION MOTIVATION MODEL[1]

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I shall hold that the development of adequate concepts of motivation and cognition—concepts which will be useful to all the social sciences and not merely to psychology—will be helped by the adoption of a diagrammatic psychological model. The virtues of such a model are, as I see it, two: (1) a diagram, or a set of diagrams, allows the simultaneous presentation of a large number of interrelationships which it would take several pages to state serially in words or equations; and (2) in the working out of such diagrams one is led to conceive of types of interrelationship which may be very important and which yet would be less obviously thought of, if mere words or equations were used.[3]

The specific model I shall propose contains three basic constructs which I shall call, respectively: (a) the need system; (b) the belief-value matrix or belief-value matrices; and (c) the immediate, concrete behavior space. Let me begin by talking about (b), the belief-value matrix. Figure 1 presents a small partial matrix. It presents "belief" relationships only.

For the purposes of exposition I am leaving out the accompanying "values" until subsequent figures. The behavior of any mature individual is conceived as being determined in part by a very large number of such small belief-value matrices interlocking, overlapping and including or excluding one another. A very multidimensional diagram would thus be necessary to indicate at one and the same time all these assumed interlocking and overlapping matrices of beliefs (and values).

The cells (squares or circles) in Fig. 1 represent "typed" (i.e., not concrete) "images" of objects or environmental situations, which the individual is conceived to carry around with him as a result of previously learned categorizations and differentiations *re* environmental objects. The cells indicated on any one vertical line represent a set of "similars." And each vertical line itself represents a "generalization dimension." The particular matrix represented in Fig. 1 contains a generalization dimension *re* restaurant types and another *re* types of objects-to-be-eaten, or foods. The units of these dimensions, if we had them, and the relative spacings along them of the object images would define the degrees of "functional" similarity between such objects—that is to say, the degree of similarity between the different kinds of restaurant relative to the function of leading to foods or between the different kinds of food relative to the function of leading to hunger gratification. They

[1] This is an abbreviated statement of essentially the same position as that developed in greater detail and presented in a chapter entitled "A Psychological Model" in the joint volume, *Toward a Theory of Social Action*, by Talcott Parsons *et al.*, Harvard University Press, 1951. The writer wishes to express his especial indebtedness to Dr. Leo Postman for help in the development of many of the concepts here presented. This paper, with minor modifications, has been read to graduate students in psychology and/or the other social sciences at Harvard University, the University of Minnesota, Stanford University, Northwestern University, and the University of Chicago.

[2] The paper was put in final form while the writer was Visiting Professor of Psychology at the University of Chicago in the spring quarter of 1951.

[3] The writer wishes to express his thanks to Mr. I. Goldiamond for help in conceiving and drawing the particular diagrams contained in the present paper.

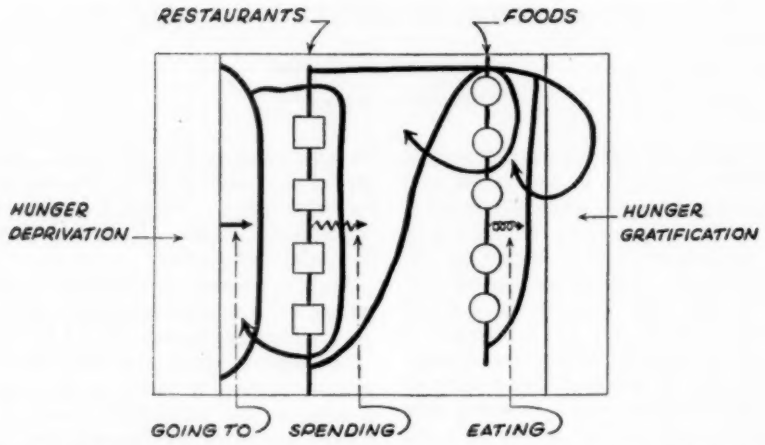

FIG. 1. An individual's, or a group's, belief-value matrix (values omitted) with respect to restaurants and foods.

would define these functional similarities as they would operate in helping to determine the choice-behaviors of the particular individual. It is our lack of knowledge concerning all the types of such generalization dimensions, the nature of their units and the spacings of the specific images along them, which is, I believe, one of the reasons why psychology is today so frustrating. We have at present little theoretical or empirical knowledge, save in the cases of simple sensory qualities, as to what the important dimensions and units of generalization are and how the specific subvarieties of objects are spaced in terms of these units. However, a recent important contribution toward the discovery of a new functionally important generalization dimension with which many are familiar lies in the work of Sears and others (2, 3, 6, 7) on doll play. It has turned out that (for the purposes of *aggression*) first the child itself, then the parent doll of the same sex, then the parent doll of the opposite sex, then the child doll of the same sex, then the child doll of the opposite sex, then the baby doll, and finally, imaginary natural forces or "spooks" turn out to be arranged in this order on a single functional dimension. If the child is high in aggression, and if there is little conflicting anxiety, he will himself express the most aggression in his play, make the parent doll of the same sex express the next most, the parent doll of the opposite sex the next most, the child doll of the same sex the next most, the child doll of the opposite sex the next most, the baby doll the next most and imaginary forces such as spooks or winds the least. This, then, is an empirically discovered new type of generalization—a dimension *re* aggression determinative of the behavior of these children—which is presumably a product of our culture. Children growing up in a culture having a different family constellation from ours might well show a quite different ordering of their dolls for the purposes of aggression. Obviously many other such types of generalization dimensions are what we psychologists would like to find.

Turn now to the looped arrows. These looped arrows (or we might call

them lassos) represent what I shall call "beliefs." These belief lassos are a very important constitutive feature of any matrix. The forked ends at the beginning or left of each arrow, or lasso, represent a generalization spread as regards the type of initial (*terminus a quo*) object or situation. The spread and shape of this forked end of a looped arrow represent the range of types of initial objects or situations which will be accepted, and with what respective readinesses, as appropriate means objects for supporting a given sort of behavior (indicated by the small arrow) in order to get to such and such a further, *terminus ad quem* type of object or situation. And the inclusiveness or spread of the far end of a loop indicates the degree of generalization involved in the belief as to the types of end-object or situation which will be achieved by this given type of behavior. Concretely, this diagram represents a hypothetical case in which an individual (obviously one in our society) has come to *believe* that, if he is hungry (i.e., if he is in the presence of hunger deprivation), he should "go to" such and such types of restaurant and that these restaurants when responded to by "spending" will get him, with such and such respective readinesses, to such and such types of food and that such and such types of food, when responded to by "eating," will get him finally, with such and such respective degrees of readiness, to hunger gratification.

But this diagram, as given in Fig. 1, presents, as I said, only the cognitive or belief side of the matrix. In actuality such beliefs do not exist apart from their motivational and value accompaniments. Thus the individual will have learned at one and the same time not only that certain foods when eaten lead with such and such degrees of probability to hunger gratification, but also that such gratification has positive value and that hunger deprivation has negative value, and hence that such and such foods and such and such types of restaurant also have varying degrees of positive value. In other words, a complete diagram of any actually existent matrix must contain not only beliefs but also values—that is, goodnesses or badnesses deposited on the individual cells. In Fig. 2 I have added, therefore, plus and minus symbols to represent these values. I have indicated that one type of food is actually believed to lead to hunger deprivation and hence to have negative value.

A further point to be noted about Fig. 2 is that it may be conceived to represent (for a very simple area) a culturally and sociologically determined belief and value system as internalized in the given individual. This belief and value system, this matrix, if correctly inferred by the observer, would be statable in verbal propositions such as:

1) If X (the individual in question) was positively valuing hunger gratification and correspondingly suffering hunger deprivation, he would value such and such types of foods in such and such an order by virtue of his belief concerning their respective properties for producing hunger-gratifications.

2) If X valued positively such and such types of food, he would be ready to value positively such and such types of restaurants in such and such an order because of his belief that these types of restaurants would lead with such and such degrees of probability to such and such types of foods through the act of spending. He might, of course, also value such types of restaurants negatively because of his beliefs concerning the negative values involved in having to spend money. To represent this latter complication another belief-value matrix *re* spending would have to be added to the diagram.

Further, if questionnaires and verbal reports can be considered reliable forms of data, these propositions about the

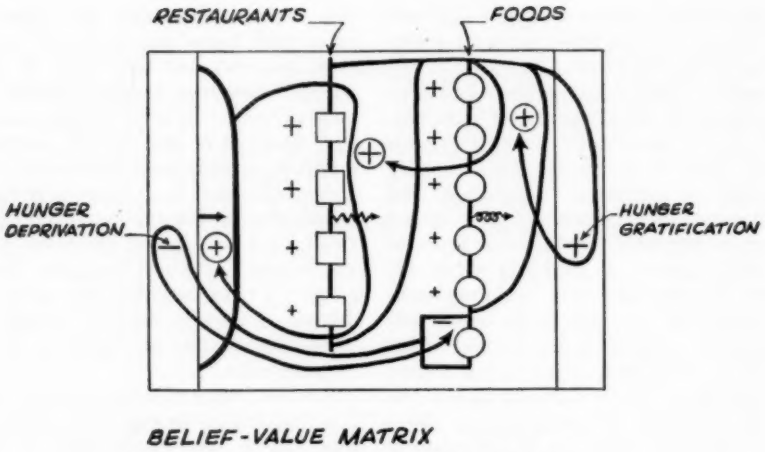

FIG. 2. A belief-value matrix, including values.

matrix or matrices could be relatively adequately inferred from such questionnaires or interviews. Thus, for example, we could ask a subject:

1) "What are you ready to do when you haven't eaten for a considerable length of time?"

2) "What kinds of food do you like? Name six varieties of food in order of preference. What do you like about each of these six?"

3) "For each of these six foods what types of restaurant would you go to and in what order? List all the considerations you would take into account in choosing the one kind of restaurant or the other."

The use of such "*attitude questionnaires*," although, of course, far better constructed, or the use of interviews would thus be one general kind of operational technique for getting at the contents of a belief-value matrix. The adoption of such a technique involves, of course, two basic assumptions, neither of which is probably wholly valid. On the one hand, the use of questionnaires or interviews assumes that under the impetus of some other drive such as pure intellectual curiosity, or perhaps merely the desire to please the experimenter, the individual can, so to speak, run over his matrix and report it verbally without producing any serious distortions in the matrix structure itself. And, secondly, the use of these techniques also assumes that the subject's matrix is all verbally available to him—that is, that no essential parts of the matrix are, to use Freudian terminology, "repressed." Actually, however, we know that important features of many crucial belief-value matrices are in fact unavailable for verbal report. Thus, for example, we may find ourselves, or others, possessing very strong positive or negative values about killing, about war, about Communists, about Democrats, about Republicans, or about signing oaths, in cases in which the underlying beliefs and their generalizations which would explain these values are in large part hidden. Yet the analytical techniques of depth psychology can nevertheless often finally bring such connecting and explanatory beliefs and generalizations to light. Projective techniques can likewise be used to uncover hidden portions of the matrices. However, I would as-

sert that the results of both clinical interviews and projective techniques must in the end be validated against direct observations of resultant actual behavior.

Finally, one further word about matrices. As social scientists, it is to be noted that we may operate with three different levels of matrix. First, we may be interested in a matrix in the sense of a statistically average or modal matrix relative to given types of environmental situations shared by a whole group of similarly placed individuals in a given society or culture. Such a modal matrix, shared by a whole cultural group of individuals and which concerns the most basic and most general features of their environment, is indeed, I believe, what anthropologists have sometimes called the *ethos* of a culture. By conceiving such an ethos as a belief-value matrix I am, I think, merely pointing to a more precise and less intuitive way than that usually used by the cultural anthropologists themselves for stating just what such an ethos consists of in the way of actual differentiations, generalizations, beliefs and values.

Secondly, there would be the statistically average, modal, matrices which are characteristic of the particular individual. These would state the characteristic, or modal, refinements of differentiation; the characteristic, or modal, spreads of generalization; and the characteristic, or modal, beliefs relative to means-end relations and resultant values with respect to given types of goal-objects and given types of means-end activity which would hold on the average for the given individual. It may be noted in passing that if the particular individual be a very average and very conforming individual, his modal matrices will coincide very closely with the corresponding modal matrices shared by his society as a whole, or at least by most of the other individuals in the same sort of social position in that society that he himself is in.

Finally, thirdly, there would be the activated, determinately valued matrix of the individual on the particular occasion resulting from the precise stimulus situation and the specific drive arousals at that moment. Thus whereas an individual's modal matrix would be defined in terms of the modal magnitudes of his differentiations, generalizations, beliefs and values relative to the given type of activity and the given type of goal, his activated matrix on the particular occasion might, as a result of the particular intensities of his then-and-there aroused drives and of the particular characters and intensities of the stimuli then and there presented to him, contain types and magnitude of differentiation, generalization, belief and value which might be somewhat different from those in his corresponding modal matrix. Thus, for example, if the individual's hunger drive on the given occasion was either excessively strong or excessively weak, the refinements in his differentiations of types of foods might be far less than would be true in the case of a more average magnitude of his hunger drive. Similarly the frequent or emphatic presence in the environment of stimuli corresponding to a particular type of food or a particular type of restaurant might on the given occasion lead to a greater positive value for this food-type or for this restaurant-type than these normally would have.

Turn now to Fig. 3. This diagram indicates that for the final predicting of an actual behavior of a particular individual on a particular occasion we have to know not only the nature of his governing matrix or matrices but also the momentary stimulus and drive conditions to which he is then and there subjected. For these drive and stimulus conditions will determine the then-and-there magnitudes of the individual's

needs and hence of his final goal values. Furthermore, as we have just seen, they may also change slightly the form of his matrix itself—causing it to be more or less differentiated than his corresponding modal matrix. Further, it appears that this activated matrix operates conjointly with the presented stimuli and the autochthonous principles of perception to determine his concrete behavior space, i.e., his perceptual, memorial and expectational processes of the moment, including his need pushes and his valences. It is this behavior space which is conceived to lead, as a consequence of the resulting behavior-force vectors evoked in it, to the "locomotions" of the "Behaving Self." And it is these locomotions in the behavior space which directly produce the externally observable behavior, B.

Lastly, it is to be noted that the resultant behavior may tend to change the external and internal situation for the individual, so that his behavior space, and, ultimately, his matrix, and even his need system, may be different on future occasions. This changing of his behavior space and of the matrix may, in the case of simple immediate behavior, consist merely in additive learning, that is to say, in the strengthening of the immediate expectations of the beliefs which were already operative. In other cases involving more restructuring, the changing of the behavior space may lead to the development of wholly new features in the matrix and, finally, such changes and new features in the matrix may lead to the establishment of new or enhanced needs in the Need System.

But let us examine Fig. 3 in more detail. Let us consider further the need system, upper left. This is conceived as a set of interconnected needs. Each need in this set or system, when aroused, is assumed to be a source of both positive and negative values—positive values for need gratification and negative values for continuing need deprivation. These positive and negative values in the needs are indicated by the little plus and minus signs. Different strengths of arousal would be represented by different numbers of such signs. The big need at the left may be thought of as a libido or a source of general energy. This is conceived to be in contact more or less directly with all the specific needs. The specific needs here depicted are *H*, hunger; *Pa*, palatability; and *Sp*, controlled output or "spending." The bifurcating causal arrow from the need system to the two ends of the matrix represents the activation of the positive and negative values of hunger need-gratification and hunger need-deprivation at the moment. The symbol D at the left, and underneath the need system, represents the physiologically defined drive conditions which, together with the environmental stimuli, SSSS, are conceived to arouse the needs. An aroused need or an aroused set of needs tends to activate any matrix to which it or they are attached. And this activated matrix or these activated matrices act along with the environmental stimuli, SSSS, to determine the concrete behavior space which will be evoked. This concrete behavior space consists of the immediately "perceived," "remembered," and hence, "expected" environmental objects plus their positive or negative valences, if they have any, and the immediately perceived and expected "position" relations of these environmental objects to one another and to the behaving self. The behaving self is represented also as containing a need-push which comes from the need deprivation in the governing matrix. I have represented the need-push in the present instance as being primarily that coming from hunger. And I have represented the rest of the behavior space as including perceptions of two concrete restaurants x_1 and x_2 with the expected

amounts of spending involved in getting to foods a_1 and a_2, respectively. To sum it up, the behavior space, as drawn, represents the fact that the individual perceives himself as facing two perceived concrete restaurants x_1 and x_2 both of the general type, X, and each as having a positive valence, and also the fact that he expects to reach such and such concrete instances of foods a_1 and a_2, each with its positive valence by similar amounts of spending, and finally the fact that he expects these foods to lead on through similar concrete amounts of eating to hunger gratification, which possesses final positive valence. As a result of all this the behaving self experiences or, perhaps better put, "suffers" behavior force vectors, shown by the two little arrows at the left, tending to cause it to "locomote" towards each of the two perceived restaurants.

In Fig. 3, as I have drawn it, I have assumed that the behaving self is faced with a relatively new situation. I have assumed that the particular specific restaurants x_1 and x_2 both of type X have never been experienced before and that the expectations as to resultant spending, foods, etc., in these two different restaurants are about equal. In other words, I have assumed that the individual's previously established connections with the stimuli are not such as to evoke essentially different perceptions and expectations with respect to the two restaurants. He will expect both restaurants x_1 and x_2 to cost about the same amount. And he will expect both of them to lead to about the same foods a_1 and a_2 both of the same type, A. They will have equal valences and his behaving self will be pushed by about equal vector forces towards both restaurants. He will in the case diagrammed go towards one or the other restaurant almost by chance. But, later, after having thus behaved over a number of oc-

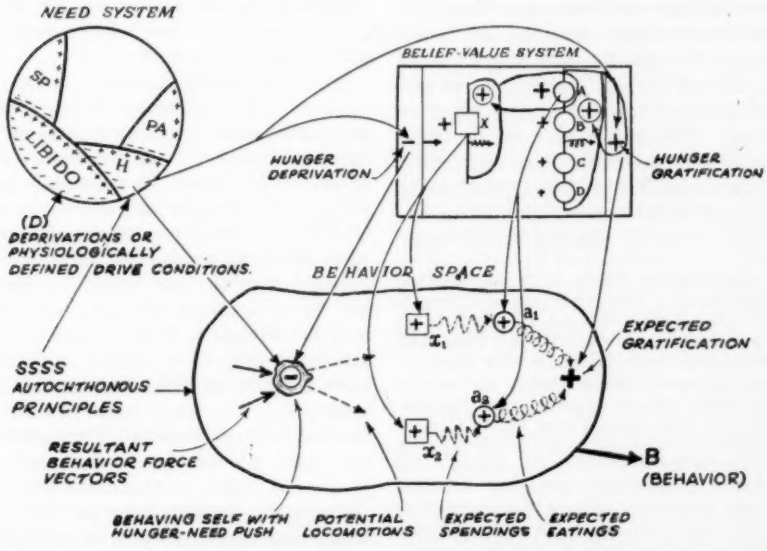

FIG. 3. Independent variables (D and SSSS); intervening variables (need system, belief-value matrix or system and behavior space); dependent variable (behavior, B).

casions, he may find out more precisely that one restaurant is better than the other and that they lead to somewhat different foods. That is to say, he may learn. Or, in other words, he may tend to establish new, more precise perceptual discriminations and expectations in his behavior field, and these in turn may lead to more precise differentiations and beliefs in his matrix relative to subtypes of restaurants and relative to subtypes of food, respectively, to be obtained from such restaurants and relative to the goodnesses or badnesses of these foods for producing hunger gratification. On subsequent occasions in which he is presented with the same stimuli these may lead to a more precisely differentiated and, we may hope, a more veridical behavior space.

Such learning takes place, I believe, not primarily through reinforcement in the Hullian sense but merely by the *repeated "confirming"* experiences of finding what restaurants lead to what foods by what costs and with what degrees of final gratification.[4] Further, this learning will through the laws of generalization spread to other similar types of restaurants and to other similar types of foods. The individual's matrix may, through the principles of generalization, become expanded to include positive and negative values for new, but more or less *similar,* types of restaurants and foods.

[4] Such learning will, no doubt, be somewhat faster up to a limit with increasing hunger. Too great hunger will, on the other hand, probably interfere with learning. *That* food and *that* restaurant will come to be "gone to" more frequently which produce the greater satisfaction with the least cost. But this does not mean that the character and location of the "good" restaurant are necessarily going to be learned any faster than the character and location of the "poor" restaurant. In other words, there is no evidence that greater "reinforcement" will cause the one restaurant and the one food to be learned more rapidly than the other less good restaurants or less good foods.

To sum up, a very abbreviated description of learning would be as follows. On the basis of past experience the individual brings a modal belief-value matrix to any new stimulus situation. This modal matrix becomes activated and particularized as a result of the presented environmental stimuli and the specific need arousals at the moment. This activated matrix together with the environmental stimuli lead to a particular behavior space. This behavior space will result in locomotions of the behaving self in this behavior space and these locomotions will result in actual behavior. And this actual behavior will lead to new stimuli which will either confirm and tend to strengthen, or not confirm, and hence tend to modify the original object perceptions and expectations and position relation perceptions and expectations which constituted the original behavior space. Then these confirmed or modified perceptions and expectations will react back upon the matrix and either strengthen the differentiations and the beliefs already there or create new ones.

Next I should like to raise a question concerning the nature of the so-called secondary needs. Consider, for example, "need-achievement." It is obvious that in our own culture there is a large amount of something which has come to be called by this name whereas in certain other, more primitive cultures, there is reported to be much less of it. How are we to envisage this need-achievement? We shall probably all agree that this "propensity to achieve" is, to begin with, a means-activity—that is, that it corresponds to a set of acquired instrumental beliefs and values. The individuals have learned, i.e., have acquired means-end beliefs to the effect that, if they persist in and complete difficult tasks, they tend to get praised and that, if they give up and do not complete such tasks, they tend to get blamed. Assum-

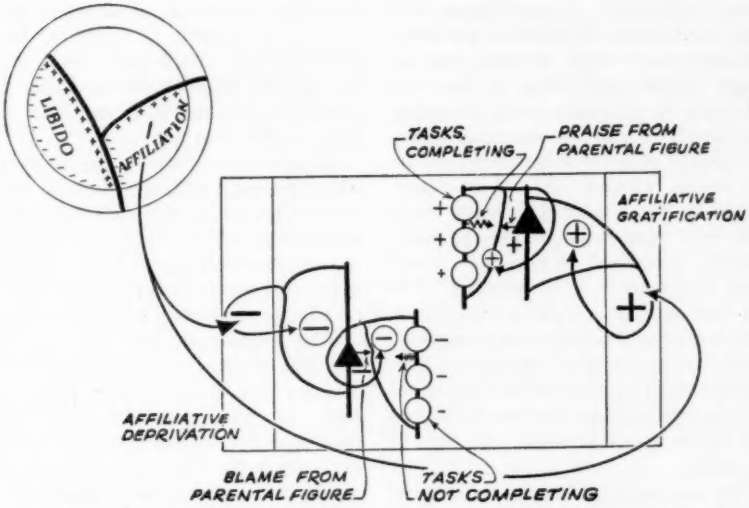

FIG. 4. Completing tasks and not completing tasks are believed to lead to parental praise and blame, respectively, and these latter, respectively, to gratification and deprivation of the affiliative (love) need.

ing some simple and probably innate need, such as need-affiliation, then the receiving of praise can be conceived to lead to a direct gratification of this need, and the receiving of blame to a direct deprivation of it. Then as a result of these experiences of completing or achievement leading to praise and of not completing or giving up leading to blame, the individuals gradually acquire two new dimensions in their matrices: a completing dimension and a not completing dimension. Positive instrumental values will get attached to the cell or cells along the completing dimension and negative instrumental values will get generalized to the cells along the non-completing dimension. In other words, the individuals can be assumed to build up a matrix like that diagrammed in Fig. 4.

Furthermore, it would appear that by the time they are adults, individuals who have been trained in a given culture where praise and blame were consistently, *or perhaps inconsistently*,[5] handed out for persistence and completing, on the one hand, and for giving-up or not completing, on the other, will tend to continue to "go in" for achievement, even though nobody appears any longer to praise or blame them for it. This suggests a further possibility in terms of our model: namely, that in time a really new need sac—to be called need-achievement—gets set up in the need systems of such individuals. This possibility is diagrammed in Fig. 5 which includes a new "functionally autonomous" need achievement.

If this state of affairs is in fact finally arrived at, it would mean that such individuals should, and would, keep on "achieving" even though neither actual persons nor imaginary ones gave them approval and also even though no viscerogenic or other constitutional needs were especially furthered by such con-

[5] This latter possibility has been suggested by McClelland (4).

tinued achieving. It would mean that these individuals, if placed on an island paradise where work, as such, was no longer valued and where it was *not* necessary to do much work to satisfy the bodily wants, would nevertheless continue to go in for achievement *per se*. To decide whether the set-up represented in Fig. 4 ever thus really turns into that represented in Fig. 5, many actually controlled observations would have to be made. Furthermore, if it did turn out that a truly new need such as this need achievement were generated, careful observations of the original conditions which favored the building up of such a new need in the one individual but not in the other would also have to be made.

My own personal hunch, at the present time, is that no such new need sacs are really established, but rather that all we really get are very powerful instrumental values connected by many and relatively strongly acquired belief lassos to a wide variety of ultimate, basic gratifications. If the basic needs could be satisfied without the activities involved in these so-called new needs, the latter would drop out.

Another point:—Some psychologists, who are greater students of social interaction than am I, may feel that I have not allowed sufficiently for the fact that the objects which appear in the behavior space and which get generalized in the matrix are not merely inanimate objects such as restaurants, foods, and the like, but also other persons and the complementary responses of these other persons. My answer would be that all expectations and beliefs, even those of the rat, include from the very beginning the resultant complementary responses of the other objects, whether these latter are animate or inanimate. The expectations and beliefs of "ego" in relation to "alter," whether that ego be rat or man,

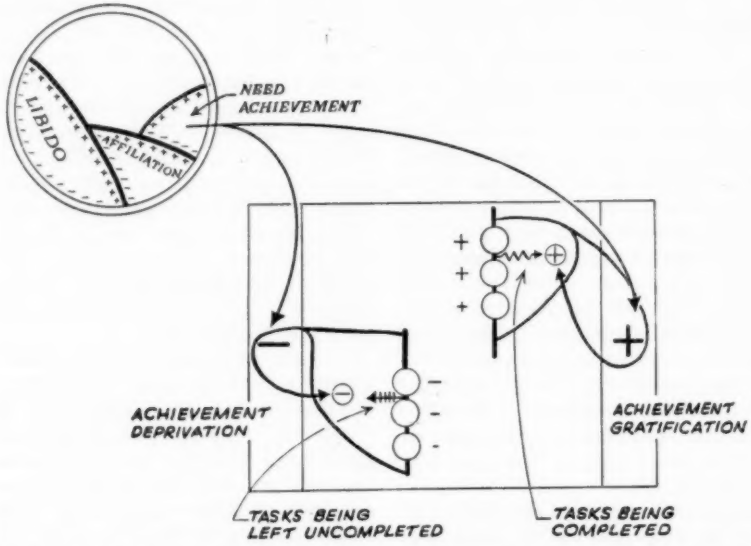

FIG. 5. Contents of need system and matrix on the assumption of an acquired, functionally autonomous, need achievement.

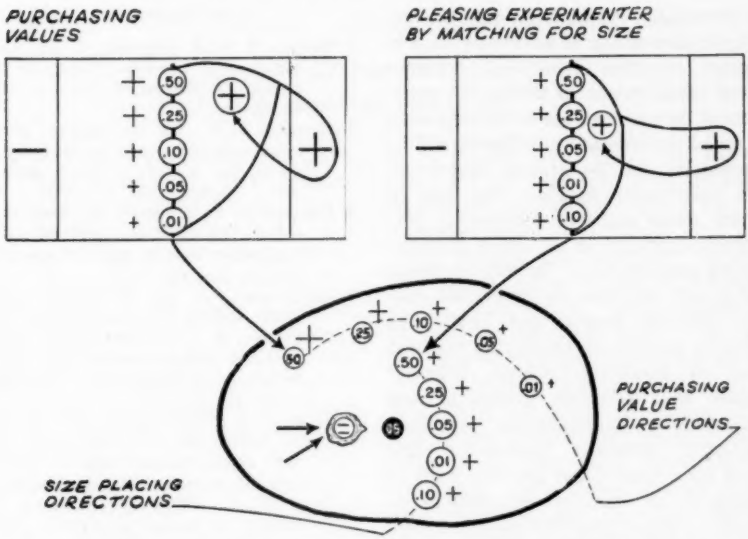

FIG. 6. Suggested explanation of Bruner and Goodman results. Matching of five-cent piece for size distorted by simultaneous (unconscious?) matching for purchasing value.

are expectations and beliefs as to what ego will receive in the way of return reactions from alter, whether such alter be a person or an alley wall.

Still another point:—I do not have space here and I have not thought it through far enough to be too assertive about it, but I believe that the so-called Freudian mechanisms can be treated most fruitfully not as dynamic peculiarities in the need system or in the matrices of the subject but as relatively inevitable types of resolutions of conflicts in the concrete behavior space when the behaving self is simultaneously pushed both towards a given object and away from it. The type of defense mechanism that will result will depend more upon the nature of the objects and their relative then-and-there positions in the behavior space than upon any innate predispositional feature in the need system or in the belief-value matrices of the given subject.

Finally, I believe that very fruitful analyses of the distortions of the perceptual behavior space which are appearing in recent experiments such, for example, as those of Bruner and Goodman (1) in which, for example, in poor children the more valuable coins were actually seen as larger (this has been shown by Rosenthal [5] to hold only for certain ages under certain special conditions), can be most usefully conceived as a case in which both a matrix which arrays the coins according to their monetary worth and a matrix which arrays the coins according to their physical sizes both project onto the same behavior space. This possibility is diagrammed in Fig. 6. The locomotion of placing the coin relative to size is distorted by a placing of it relative to purchasing power.

One last word: I should like to emphasize that it is my firm belief that all of the so-called dynamic problems

of personality psychology and the resultant phenomena of selectivity of perception, cognitive distortions, and emotional break-downs, as well as the problems of the maintenance or disintegration of social systems and of cultures, will be illuminated by the general concept of the belief-value matrix. The way in which whole matrices, whether in personality systems or in social systems, govern part matrices; the ways in which these part matrices do or do not conflict with one another and, finally, the ways in which such conflicting matrices lead to resultant concrete behavior spaces with resulting distortions and consequently "inappropriate" or "maladjusted" resultant locomotions and final behaviors—these are the ways in which I, at least, would attempt to conceive and to state the basic and most important problems for the understanding of society and of the individual.

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THE STATISTICAL THEORY OF THE FIGURAL AFTER-EFFECT

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In 1948 there appeared a brief paper (7) which outlined a number of rather basic objections to the satiational theory of the figural after-effect (*cf.* 2, 5). The paper is mentioned in the standard reviews (8, 9), but it has never been discussed, as far as the writer knows, by satiation-theorists.

Now Osgood and Heyer have presented a new interpretation of figural after-effects, an interpretation which they have called the "statistical theory" (6). Recognizing some of the well-known inadequacies of a pure field-theory of cortical action, they have reconstructed the essence of satiational theory within the framework of conventional neurology. Their exposition is logical and lucid, and what emerges is a theory which is perhaps far more elegant and persuasive than the older one. It must be pointed out, however, that the theory of Osgood and Heyer is enough like that of Köhler and Wallach (2, 5) that each of the objections previously raised against the latter may be raised with equal force against the former.

The criticisms were six in number. They may be recapitulated here, each paraphrased in terms of statistical rather than satiational theory:

(1) After-effects essentially similar to figural after-effects are obtained in the waterfall illusion and with the Plateau spiral. In neither situation is there any selective adaptation which might provide a basis "even for static displacements in the test-field, let alone for the obvious dynamic after-effects which are commonly observed" (7).

(2) Gibson originally found "figural after-effects" in subjects who had worn distorting spectacles for several hours (1). In this case, as in that above, there was no opportunity for the configurational adaptation required by the statistical theory.

(3) Gibson also obtained "figural after-effects" when his subjects merely viewed curved-line inspection-figures (1). Again there was little if any opportunity for configurational adaptation.

(4) After-effects in modalities other than vision are common. Gibson (1), and Köhler and Dinnerstein (3), have demonstrated such effects, especially in kinesthesis. The latter authors have aptly remarked that the same theory which explains visual effects should also explain kinesthetic, but that ". . . [after-effects] in kinesthesis appear at present almost inaccessible to the theorist" (3, p. 220). This statement holds for statistical theory as well as it does for satiational.

(5) Figural after-effects occur perfectly well across the vertical median of the eye, and thus across the median longitudinal fissure in the striate cortex. As Osgood and Heyer observe, ". . . both statistical and field interpretations of after-effect phenomena flounder over this apparent gap in the projection system" (6).

(6) Köhler and Emery reported visual after-effects in the third dimension of space (4). It is difficult to see how a "distance" can become adapted in the striate cortex. Here again Osgood and Heyer recognize a difficulty; they resolve it in an expression of scepticism as to the existence of true third-dimensional effects (6).

These are difficulties which seem to arise in connection with both the satia-

tional theory and the statistical theory of figural after-effects. Against the statistical theory alone, however, one must demur still further. Being the more detailed and explicit formulation, it is also somewhat the more vulnerable. It makes two specific predictions neither of which is fulfilled:

(1) It follows from statistical theory that an inspection-contour which is exactly coincident with a test-contour should not have a displacing effect upon the test-contour (6). Yet in Köhler and Wallach's basic demonstration, where the inspection-contours *are* congruent with the test-contours, displacement of the latter is commonly reported (5, p. 271). Osgood and Heyer, incidentally, reproduce this demonstration and describe it as one in which displacement effects never occur (6, p. 106). In another connection (5, pp. 281–284) Köhler and Wallach undertook laborious experimentation to determine whether or not coincident inspection-contours are as effective in displacing test-contours as are non-coincident ones. Such labor would hardly have been necessary if, in truth, displacement ". . . [*reaches*] *zero when the T*[*est*]*-contour coincides with the previous I*[*nspection*]*-contour*" (6, p. 106; italics in original).

(2) It also follows from statistical theory that test-contours cannot possibly move perceptually *toward* inspection-contours (6). In point of fact such movement sometimes occurs. Köhler and Wallach mention it explicitly (5, p. 297), and the writer has on file protocols of several rigorous observations by himself and by others in which this anomalous effect showed itself.

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